PSYCHOPATHOLOGY AND SOCIAL PSYCHOLOGY

PART III. HIERARCHIES OF INTERESTS

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I. INTRODUCTORY

People quite obviously differ in the kinds of interests they have; a man may be a passionate devotee of some pursuit that his neighbour thinks is pure nonsense. But, while granting that there are many exceptions, we are probably safe in saying that the major differences arise from variations in the weight laid on interests common to most people. Most of us have professional, political, religious, domestic, recreational, aesthetic and intellectual leanings, but no two of us will rate them in the same order of importance. Two people may meet in the same office, the same club and the same church, and find themselves voting for the same party, but, if the life of one seems to revolve about his church and the other seems to think chiefly of making money, they will be quite different people. Personality, then—apart from differences of physique, temperament and native intelligence—is a matter of the relative values placed on interests that are widely distributed. This rating is often called a *scale of values*.

II. SCALES OF VALUE

If we could work out accurately the scale of values of either an individual or a group, behaviour would become predictable. Unfortunately, accuracy is unobtainable for two reasons. The first is that few psychological data can be reduced to numerical terms. We may be able to say that so and so is more of a politician than a business man but we cannot say that the proportion of these is 5:2. As we have seen, external events may stimulate the activity of one interest, temporarily exalting its relative importance. If we could measure the preoccupation with politics and business, we could say that a business opportunity might double business activity but it would still be less intense than the political interest, while, if it were trebled, politics would be put second. But such calculations are quite impossible. The second reason for inaccuracy is that, even if we could measure interests at any given time, the measure would be valid only for that period of life. What is absorbing at 15 may be dull to the man of 25. At the latter age some interest may be engrossing that was only rudimentary at 15. So, to make truly accurate predictions, we should need not merely cross-sectional measurements but also others which represented rates of change. Not only is such measurement impossible to-day, but there is not the slightest indication of its ever being feasible.

Difficulties of this kind are, however, not peculiar to psychology. They confront biology at every turn. One of the peculiarities of life is that so many of its manifestations can be

described only in qualitative and not in quantitative terms. But this is no reason for defeatism or for illogically denying the existence of qualities simply because they cannot be measured. A man who values liberty more than life is quite a different person from one who would abandon all claim to independence for the sake of security. So we can compare in value what we cannot rate in numerical units. Thus it is possible to erect a scale of values which gives a description of a personality and which enables predictions of behaviour to be made with some hope of their being fulfilled.

Experimental psychologists have devised schemes for rating values by questionnaires, the putting of hypothetical dilemmas and so on. Such investigations are better than none but they all suffer from an important source of error: they assume that the subject knows himself, which is often far from the truth. To take a classical example: Saint Peter was presumably sincere when he said he would die before he would deny his Lord. But how false his self-rating was appeared within a few hours. The clinical type of investigation gives more reliable results, although—alas—it is a tedious process. The clinician does not exclude a man's estimate of himself but regards it as only one datum that must be correlated with many others before any conclusion can be formed. There are at least five other criteria that betray the importance of one interest as against others. These may be briefly explained.

III. Criteria of Interest-value

(1) *The element of compulsion*

The first criterion of interest-value derives from a somewhat paradoxical feature of interests. Evolved from appetites or drives, they retain the *primitive element of compulsion* regardless of expectation of pleasure or discomfort. Hedonism seems to be an explanation of much human behaviour only when sophisticated rationalizations are assumed. By far the majority of actions expressing an interest are not themselves interesting; they are part of a dull routine. Is a housewife elated as she washes the dishes or sweeps the floor? Does she look forward to these tasks in happy anticipation? Why does the business man, who is his own master, not yield to the temptation to play golf on a fine summer's morning? The hedonist, of course, will say that the subject is drawn to his task either because he looks forward to the pleasure of successfully completing it or because he fears the evil consequences of truancy. Now here is a straight question of fact. Does the free agent, engaged in an occupation which he once chose for himself, actually go through this reasoning or does he not? Common experience would seem to give the answer in the negative. Some modern hedonists, it is true, are prone to argue that one is moved by an *unconscious* fear or pleasure. Now one may automatically (i.e. unconsciously) avoid danger or satisfy a simple craving, but an emotion that acts as a motive must be consciously felt. So this explanation implies dual (if not multiple!) personality; it assumes the presence of another self capable of feeling and thinking. Since only a few score of such cases have ever been recorded, all highly pathological, it appears a poor explanation for a phenomenon that is well-nigh universal. The fact seems to be that the bulk of our behaviour is habitual, undertaken without expectation of joy or sorrow; we have chosen certain occupations which we follow, according to our natures, with great or little assiduity just because they seem the natural things to do. Originally, perhaps, they were undertaken for reasons that could be fitted into the hedonistic framework but, once they have become a matter of routine, we think no more about why we go on working. To work at this or that has become part

our nature; to default in our accepted tasks would be to be untrue to ourselves.[1] The personality is maintained unquestioningly much as is respiration. We do not breathe because we fear suffocation; we just go on breathing.

How a man spends the bulk of his time gives us, then, some indication of what manner of man he is. We speak of one who gives all his energies to one undertaking as having a dominant interest. Of another we say that he has varied interests—or that he has a 'full personality' because he apportions his activity in varied pursuits. Or we describe the miserable soul who never sticks at anything long, who is always being distracted to a new business or a new hobby as being a weather-cock, being weak-willed, or as having a weak personality. He lacks the inner compulsion which maintains the sturdier interests of his more normal fellows. A less usual way of describing such a person would be to say that he had no discernible scale of values. No one preoccupation governs his thinking more than another.

Analogies to the dominance of interest in the individual are not lacking in social psychology. An obvious example is in 'total war'—a term that is often incorrectly applied to attacks on civilians as well as on armed forces. The term was invented to designate the enrolment of all citizens, civilians and soldiers alike, in war effort. The dominance of this interest, when a war is in progress, is manifest. War news takes first place, people work harder than in times of peace and often do so for none but patriotic reasons. Everyone wants to labour at something that contributes to winning the war and the greatest social prestige is accorded to the tasks that have an obviously military significance. This, however, merely demonstrates what happens to a nation when forced to defend itself or is inflated with ambitions of conquest. It tells us little about the normal character of a people. This appears rather in their behaviour in times of peace.

It is possible to assess national character crudely at least, on a quantitative basis. Money buys goods and services and, directly or indirectly, goods are not made or obtainable without labour. So how money is spent gives us a measure of the amounts of work that are considered appropriate to various activities. What proportion of the total national income is spent on fighting services, on education, on religion, on social services, on art and on recreations of various kinds, as compared with what is allotted to food, clothing and housing? The last three measure the standard of comfort; the others reveal how the community as a whole rates activities beyond those devoted to the satisfaction of individual creaturely needs. Is the visitor struck by the number of barracks, the number of schools, the number of churches, the number of art galleries, the number of theatres or the number of sports grounds?

(2) *Intensity of emotional reaction*

The second criterion is that *the greater the dominance of any interest the greater is the liability of events that affect its prosecution to provoke strong emotional reaction.* This phenomenon stands in contrast to the relatively emotion-free character of the quasi-compulsive activities of day-to-day life. The woman who addresses to her children more words of reproof than of affection is plunged into fearful gloom when one of them is taken ill, or is elated beyond measure when one of them is successful in some competition. An author who suddenly gets an idea for a story is carried to a seventh heaven of excitement and no

[1] This may explain, in part at least, the extraordinary persistence with which most workers maintained their habitual routine during the Blitz (Ed.).

success in any other field can give him comparable pleasure. Adverse criticism of his work will cast him down as would no aspersion on his virtue. Phenomena of this kind are so notorious that the point need not be laboured. It will be enough to suggest that the layman, in estimating the character of his neighbour, probably uses this criterion more than any other. The other chap is clearly different from oneself because he takes so seriously something that one regards as trivial.

That groups betray their communal scales of value in emotion is less obvious but none the less demonstrable. One series of events in this country may be cited to illustrate the principle. What does the monarchy mean to the people of this country, to the Empire and the Commonwealth? The powers of the King have been so whittled away that a forceful back bencher in Parliament may actually enjoy more political power. So, one might say, the monarchy is just a symbol, an emblem like the flag flying over the Houses of Parliament. Or, if the King seems to exert a personal influence, the superficial observer might ascribe it to his personal popularity. But recent history excludes these easy generalizations. King George V was a man whose temperament inclined more towards austerity than *bonhomie*; it was the Prince of Wales whose winsome smile captivated the masses. Yet the Jubilee brought a general rejoicing too enthusiastic for it to have been produced by propaganda. A few months later he died and sorrow, as if for personal loss, swept round the world, not merely through the lands where the Union Jack was at half mast, but even in the United States. No one lightened the gloom by saying that now we were going to have as King a man who was a friend of the people. No one wanted change. Then came the abdication crisis. The light of an all-too-human figure—who had recently been held to be the most popular man in the world—went out like a snuffed candle. A wave of resentment, mitigated by a surprisingly ineffective sentiment of pity for the victim, swept the country. Edward VIII had betrayed his trust; he had let the Crown down. Finally came the pageantry of the Coronation and the people abandoned themselves to joy touched with a kind of quasi-religious fervour. They had a King—and a Queen—who would fittingly represent the Crown. If one flag wears out and another is hoisted in its place, few notice the change. In its emotional significance the Crown seems to be something like a deity and the one who wears it a priest. There may be good priests or poor priests but the Crown's the thing.

(3) *Choice of fealty*

The third criterion gives, when available, direct evidence as to relative positions on a value scale. Belonging as we all do to many groups and having varied interests, dilemmas frequently arise when a *choice of fealty* must be made. Some interest has to be neglected if another is to be pursued. The choice will be made without hesitation if the subject has long since rated the claims of the conflicting loyalties and has a clear picture in his mind of their relative importance. For instance, a man who has made an appointment to play a golf match will call it off should a business emergency arise on that day, but he would cancel an important business engagement if his wife were taken ill and there was no one else to look after her. In many cases, however, there has been no conscious formulation of degrees of obligation. Then there is a period of painful doubt and choice is made of what is felt to be the lesser evil, or the subject in his dilemma asks and accepts advice about a decision that he ought to make for himself. If the choice coincides with what the more unconscious levels of his personality approve, rationalizations appear which confirm the choice. If, however, it really goes against the grain of his inner nature, conflict persists and

the subject keeps worrying lest he has perhaps done the wrong thing. He may even reverse his decision. Some people know so little of themselves that they solve such problems with a trial-and-error technique. On the toss of a coin one programme or the other is adopted and a real choice is made only when the result is found to be either tolerable or intolerable. An individual without an organized scale of values has, of course, a poorly integrated personality and drifts throughout life from one agony of doubt to another. This picture is all too common among those who seek medical advice on account of neurosis or character disorders. Few people who have consistently known what they wanted and gone after it seek psychological aid.

Quite similarly, a nation may be posed with dilemmas in policy, and the decision reached throw light on its ethos. Short-term strategy and deep-lying moral principles may be in conflict. Many illustrations of this could be cited but one must suffice. I cannot do better than quote what I wrote in 1942:

> An excellent example of a significant national choice was given in the spring of 1941. One does not know what the motives or calculations of the Government were when an expeditionary force was sent to Greece, but the popular reaction thereto was observable. I heard no one express a hope of victory but many who predicted failure. Yet few of these avowed pessimists failed to add: 'But I suppose we've got to do it.' A minority said it was a mistaken policy, that we lost more prestige through defeat than we should gain by demonstration of loyalty to an ally. When I asked any of this group what he would have done had the decision been his to make, there was either an evasion on the ground of insufficient information, or else the statement: 'Oh, I suppose I should have done what the Government did.' It seems that the country did approve of this expensive gesture not as a gamble that might come off, but as something that had to be done disregarding its cost, desperate though our need was of men and materials.[1]

Scales of value change slowly in both individuals and groups. As I write these words (March 1947) Britain is becoming daily more conscious of the problem of survival. But survival of what? Foremost in men's minds is the thought of maintenance of life at a level above that of mere subsistence. Is self-preservation in this sense going to assume priority over all other considerations? It is probable that, before this appears in print, some decision will have been made by the Government, and accepted by the majority of the people, which will show whether or not traditional moral obligations are more important than expediency.

(4) *Variation in moral judgement*

Closely akin to choice of fealty comes our fourth criterion, *variation in moral judgement.* Every man has his personal scale of moral values. Some acts are positively virtuous and others villainous. In between these poles are minor benevolences and peccadillos and hosts of morally neutral actions. Particularly in this intermediate zone are there wide variations between the codes of different individuals. One man's conscience makes playing games on Sunday a sin, while the games player is horrified by the ruthless way in which the Sabbatarian will enforce a contract. Conscience, as we have seen, reflects the ego-ideal and that in turn is a picture of the individual as seen through the eyes of those with whom he feels at home—in both the literal and metaphorical senses of the phrase. Every interest presupposes a social group devoted to it. What furthers this interest is virtuous and what hinders it is vicious. A man who is first and foremost an artist will starve rather than do what he thinks is bad work and, if to avoid starvation, he begs, borrows or steals, these are but venal sins and will be condoned by his fellows. A real sin would be to turn out some-

[1] J. T. MacCurdy: *The Structure of Morale* (1943) p. 115.

thing that is shoddy, even to repay a debt. Thus we can deduce something about an individual's scale of values from his moral sense. There is honour among thieves.

Greatly as we tend to think that conscience is our guide, that we are independent in our moral judgements, there is nothing we crave more than 'moral backing' when we have to make a difficult decision. Nothing is harder than the prospect of being a moral pariah. The moralist is prone to talk about the struggle between conscience and lust, but is less apt to enlarge on the struggle of conscience to maintain its independence of social pressure. True we hear often enough about avoiding the influence of bad companions. But these are minorities of ill-repute. If one's companions happen to be the members of a cartel, a union, a profession or a political party, loyalty to this group tends to override the individual's independent moral judgement, and actions are taken or condoned that would be deplored in other settings; there may be no twinge of conscience at all. The most striking case is, of course, that seen in mob frenzies when the perpetrators of unequivocal crimes are, otherwise, men of respectability. Thus, as a man travels from group to group in the pursuit of his various interests, he tends to adopt the moral standards of each group in turn; and the degree to which he grants priority of loyalty to one circle or another is reflected in his hierarchy of moral values when he is absent from his chosen companions.

The greatest group to-day is, of course, the nation, and nations differ as do individuals in the way they rank moral principles—and in the way they modify them. Germans put obedience to authority above all other virtues and that is why they carry out orders of which they would disapprove as individuals. In our society it is obedience to principle that we tend to honour. So the conscientious objector has his rights and a government that issued orders repugnant to the moral sense of a respectable minority would be likely to fall; the majority would judge the orders as a betrayal of the ultimate principle of freedom of conscience.

Just as a man may unwittingly change his moral standards when he turns from one interest (and one group) to another, so can a nation when it moves from peace to war. While at peace, the thoughts of most citizens are directed to personal and domestic affairs within the framework of the national ethos. In so far as there are dealings with foreigners they are governed by the same code. With the outbreak of war, however, the enemy is excluded from the human group within which the code operates. So murder, theft, arson and fraud can be practised as against the outlawed. More accurately, the actions which if practised at home or against neutrals would be given these names are no longer crimes at all. Yet in spite of this apparent reversal some consistency is retained (apart from the humane pity for the stricken individual enemy), as is shown in behaviour towards a conquered people. This tends to be similar to that meted out to political minorities in times of peace. If a minority is persecuted, its property seized and its members harried by the police, then the government that so treats a minority will deal similarly with the people of an occupied country. On the other hand, in democracies in which minorities retain civic and property rights, the governments are prone to protect a conquered people—in theory at least—from violence, pillage and forced labour. This is curiously like the consistency of individual conscience when it refuses to modify its standards completely in different social settings.

Again, just as the code of one individual may change from one period of life to another, so may the moral outlook of a community from one historical period to another. In frontier life the execution of a criminal without due process of law may be sanctioned as

'Justice'. A generation or so later the same act will be called 'murder'. Many mid-Victorian leaders of society—liberal and philanthropic in their way—regarded poverty as sinful. Analogously in this century, Americans as a whole regarded unemployment as a sign of shiftlessness. If a factory had to shut down the discharged employees were supposed to find work elsewhere, by migration to another town or state if need be. By common consent the man who failed to get a job was regarded as lacking in good American initiative. Of course there was frequently unemployment among those willing and anxious to work but it impressed only those with special social and philanthropic interest. But then came the great depression lasting for some years from 1929 on. So many people with admirable records were looking for jobs and they represented such an appreciable proportion of every social class that unemployment changed from being a moral problem to one of economics. This was associated with questionings as to the validity of the entire *mystique* of 'rugged individualism'. American cocksureness and self-sufficiency had a jolt which set the people as a whole thinking seriously about their civilization as they had never done before. Indeed it is possible that historians may rank the great depression with the Civil War as one of the turning points in the evolution of the United States.

(5) *The feeling of 'reality'*

The fifth category of the phenomena which betray the scale of values is *feeling of reality*. This has already been explained as it appears in the individual. Anything which produces a strong emotional reaction feels more 'real' than does a neutral event. Seeing one man injured is more moving and therefore a more real experience than reading of a massacre that occurred centuries ago. A striking example of this principle appeared in the case of a doctor who gave up practice in favour of public health work because he could not bear to witness suffering. As an official he knew of many more illnesses than he could ever have examined for himself but he could be impersonal about people he never saw. The importance of the sense of 'reality' for the psychology of belief is discussed in the following section.

IV. 'Reality' and belief

We say that personality is characterized by the interests which are integrated to form it. It would be nearly as correct to say that personality is an expression of a man's beliefs. A belief is a hypothesis or theory that is treated as if it were a fact, although its truth cannot be proved. I know that stones are hard and cotton-wool soft because I have proved it countless times, but I believe that Britains will win through the present economic distress because the spirit of the people—a wholly intangible factor—is indomitable. This is unprovable, not because the will of the people is intangible—psychology is usually dealing with the impalpable—but because it is incapable of measurement. The U.S.S.R. also has its troubles and I recognize that the Soviet peoples have determination to surmount their difficulties but I have little faith in their success for I believe their spirit of unity is relatively weak. These beliefs are not irrational. If they were they could be disproved and they could not be defended by arguments as to relative probability. Between the certain and the impossible lies a region of infinite gradations from the highly probable to the highly improbable. We can be reasonably confident as to the occurrence or non-occurrence of events at the extremes of the probability range. For instance, we can be reasonably sure that a generation from now the English-speaking peoples will still have

some importance in the world and that the Melanesians will not have conquered the globe. But in the middle of the field, where arguments pro and con are not fantastic, weight is given to the factor which feels most 'real'. If the reality feeling is strong enough that factor becomes decisive for the believer who then acts as if he had the knowledge which alone justifies certainty.

Thus by weighting the intangible morale of the British people so heavily that it outbalances our almost insuperable economic destitution, I conclude that we are, eventually, going to get on our feet again. Contrariwise, although I admit the incalculable richness of Soviet natural resources, I am so sceptical of their political integrity and industrial efficiency that I believe the Union will break up before those resources are exploited sufficiently to provide a decent standard of comfort for the people. A Russian, on the other hand, wil probably have beliefs about the future of Britain and of the U.S.S.R. that are based on precisely the same data but are diametrically opposite to mine. British morale, having little 'reality' to him, is under-rated whereas Russian morale, which to him seems all too 'real', assures him that all political and economic obstacles will eventually be overcome. So he concludes that Britain will perish but the U.S.S.R. will flourish.

These contrary beliefs, together with many ancillary ones, make the Russian Communist a different person from the Briton. So, in general, we may say that personality is characterized by beliefs and these in turn rest on the feeling of 'reality'. The latter is obviously most pronounced in connexion with interests that rank high in the scale of values. Men are known by their beliefs as well as by their actions, morals and loyalties.

Most of our day-to-day actions are based on knowledge, but our general programme in life is based on belief simply because we cannot measure all the factors that are going to produce the future. Our course through life is, therefore, largely determined by faith. This faith depends on the weight we lay on one incalculable factor as against another, on the feeling of 'reality' that dowers one rather than another. Thus the origin of the emotion which creates the reality-feeling is important to determine.

A feature of emotions previously discussed now becomes important. It is that the *urgency* of an emotion is the product of reaction at a primitive, even a bodily, level, while its *quality* is determined by the thoughts which are present in consciousness. This is a principle often exploited by religious revivalists. In fulminating against the baser passions and encouraging confession, crude emotions are stimulated which the revivalist ascribes to spiritual influence. A thrill that is actually carnal moves the auditor whose conscious attention is directed to the religious exhortation. So he is 'moved by the spirit' and converted—for the time being.

This principle works in much the same way with political beliefs. An engineer can make plans for building and equipping a factory in confidence that the plant will be adequate because all the factors with which he deals are measurable. But a politician is always assessing imponderables. Hence he is forced to adopt 'working hypotheses'. Philosophers and scientists may be able to work on mere assumptions but they provide little stimulus for the ordinary citizen. He wants not phantasms but 'reality'. Thus to be effective, a political theory must be a belief, something that can be treated as a fact. What is hypothetical is validated by emotion. The source of this emotion is varied. Often it springs from personal attachment to one who has been a political mentor, often it arises from loyalty to a group, like a trade union or a school, that is not itself primarily political. Not infrequently, policies have become linked with patriotic fears or hopes so that emotions of the

kind which prevail during war endow an election with something of the significance of a military campaign.

But there are also issues which have personal significance for the individual yet which become endowed with wider political reference. In many cases, the personal motive remains unconscious, whereas the emotion it engenders is attached to the political programme. In other words, a political creed may represent an individual rationalization.[1] Thus the workman may become a Socialist sincerely believing that the overthrow of private capitalism will lead to greater national wealth. All the time, however, he may be governed unconsciously by cruder motives of personal gain. In the same way, the employer may see in socialism an unrealistic concept of human nature, and sincerely believe that it results in moral decline and economic disaster. But underneath there is operating the fear that socialism would mean for him and his family a loss of prestige and a catastrophic lowering of their standard of comfort. Arguments as rational as any that can be made about imponderables are vociferated by the opponents and are as convincing to the interested parties as their latent cupidity is powerful.

Apart from sex (which is usually not implicated in politics) the most dynamic emotions are those engendered by strivings for self-preservation or aggrandizement or for the preservation or aggrandizement of the largest group to which the individual grants allegiance. When patriotism is the prime motive, as in times of war, the democrat may be prepared to jeopardize his class, his family or himself in order to preserve his country—a country which includes those who are his political opponents. But when a peace-time issue has to do with distribution of national wealth as between classes, self-preservative emotions come into play; cupidity and covetousness are unconsciously aroused. This is not to say that a genuine loyalty to class is non-existent. It is, however, not of the same fundamental biological significance as individualism or nationalism.[2] If the class becomes *the* dominant group to which loyalty is given, i.e. if it displaces the nation, then treason is afoot with its inevitable fruition in civil war. Intuitive fear of this keeps the vast majority of us from elevating class loyalty to such a pre-eminence. So consciously we continue to formulate our aspirations in terms of national weal while backing them with individualist emotions.

It can be argued that a mutual misinterpretation of motives is the starting-point for the development of truly delusional thinking in rival political groups.[3] Hence the importance of realizing how sincerity—and even passionate sincerity—may develop in connexion with motives that remain hidden to the subject's consciousness although 'obvious' to his opponent.

V. Belief and delusion

As has been noted, feeling of 'reality' can be modified in mental disease so as to constitute a symptom by itself or contribute to the formation of other symptoms. Negatively, its lack produces the depressive 'derealization' state. Positively, it so validates fantasies as to make them seem 'real', thus producing delusions and even hallucinations. The latter

[1] It must be remembered that a rationalization is not necessarily irrational and that it is always sincere. For instance, the motivating power of personal ambition may energize the thoughts and actions of a statesman who feels he is acting purely as a public servant and whose policies really are wise.

[2] Needless to state, the view that nationality has a biological basis has been hotly (and justly) contested.—(Ed.)

[3] This argument was developed in some detail by MacCurdy in later chapters of his unfinished book.—(Ed.)

mechanism can be well observed in manic states in which the patient becomes engrossed in schemes that he hopes will bring him fame and fortune. At first, he plans with some consideration for the difficulties to be encountered; but, as his excitement grows, success seems more and more assured until it becomes a certainty. Once the achievement of the goal becomes (for him) a fact, the obstacles are naturally proved to be trivial, so trivial as to be negligible. In other words, the reality of achievement (which is a measure of its emotional desirability) validates the plan. Precisely this psychological process operates with Utopian schemes in the field of politics. The social benefit is so desirable that Utopia is coloured with emotional reality. Once this is accomplished the perfect society is, quite logically, attainable. So all that is needed is that enough men of good will should assay the task. These convictions have a further consequence. The critic, who points out how impenetrable and unsurmountable are the barriers between us and this promised land, cannot be a man of good will. That is, he does not want to abolish destitution, ignorance and vice; he likes the bad old world, he is a 'reactionary'.

As a psychological sequence the processes of elaborating a manic delusion and a Utopian programme constitute more than an analogy. They are identical. Is the political visionary therefore a lunatic? Not at all. If he conjured up his vision all by himself, he would be. (We see many such individual schemers in mental hospitals.) What keeps the normal man from absorption in chimaeras is that such thoughts are *unconventional*. They outrage society's accepted theories about what is 'real' and what is fantastic so that to entertain such thoughts is to make oneself into an eccentric. But if a number of people with similar yearnings congregate and pool their aspirations, no one of the dreamers is without moral backing. Together they ferment their rage against the world as it is and together they paint their picture of the future. In a word, they build up a group which has a *different sense of reality* from that of society as a whole. For them, Utopia is not an unconventional, i.e. an unreal, fantasy. The individual madman is an unfortunate whose deranged emotions, are strong enough to make real what all his neighbours say is unreal, but the member of a visionary group is believing in what his brethren also affirm. In holding to his faith he is not sacrificing his contracts with his fellow men; he is actually strengthening the bond uniting him with the society of his choice. Martyrdom for such a faith is a privilege.

So far we have discussed the validating role of emotions whose roots and stems are the basic drives of herd animals. There are also the more dilute feelings associated with humdrum daily experience, feelings so undramatic that they pass unnoted until their loss is felt in an unreality depression. My chair is real because I feel it supporting my weight; the food I see is real because when I try to eat the vision it tastes good and my hunger abates. This direct effect of experience persists in memory for some time at least. What I did an hour ago or yesterday is still vivid in my mind. But whether or not I performed some task a week ago I am uncertain. Perhaps what I remember is only the intention—something far from 'real'. But only because I believe him to be a truthful person do I grant reality to an experience a friend describes to me (unless he has such dramatic power as to make me identify myself with him in his adventure). Here we are getting into another category of 'reality'; it is not validated by direct personal experience, it is not apodeictic, but is accepted as a matter of intellectual judgement. Whether that judgement is affected by emotional factors is, for our present purposes, irrelevant. The point is that its validation is secondary and open to question in a way direct sensory experience is not.

VI. Levels of psychic 'reality'

It seems that there are many gradations in the strength of the feeling of 'reality'. At the top of the scale stands highly emotional personal experience. Then comes sensory experience, the reality of which is proportionate to the frequency of its repetition. I never question the validity of the floor I walk on but I may wonder whether I have heard some unusual sound or have just imagined it. Next comes experiences, the memory of which is beginning to fade. After that we have knowledge derived from the recital of others or from reading. The more intellectual a man's life, the larger is this category. But within the vast store of second-hand information there are big differences between our attitudes towards the items that really interest us and the ones that do not. Of the latter we are apt to say (or think): 'It may be true but it doesn't matter.' Only things that have a high feeling of 'reality' attached to them 'really' matter. On the other hand, a datum pertinent to an interest does matter; becoming part of a system of ideas, it is recalled in memory over and over again till it attains a familiarity akin to that of daily experience. Thus it becomes 'real'. An example will make this clear. We all know something about bacterial infection and something about surgical cleanliness. But most of us would prefer to dress a wound with a piece of lint that looked clean but had not been sterilized than with one that looked dirty but had been sterilized. With a doctor or well-trained nurse the situation is precisely reversed. The latter feel the presence of noxious bacteria in anything that has not been sterilized no matter how spotless it may be; they have a reality for him or her that is practically absent in the layman.

This kind of 'reality' feeling based on familiarity and interest has great political significance, especially in connexion with *isolationism*. The people and opinions that matter to us are those with which we have immediate contact, and their functional reality varies directly with the frequency of that contact. If I know no Chinese personally, never do business with them and have no interest in their history, culture or art, then what I see in the papers about events in China I take as seriously as I do the actions of Roman Emperors I had to learn about at school. Further, my knowledge of human nature (consisting largely of unformulated generalizations) is as extensive as are my human contacts. If I never move out of my village all mankind thinks and acts as do the people I know. So a Chinaman who says or does something strange is obviously a criminal or a lunatic. He doesn't need to be taken seriously. Indeed all 'foreigners' are criminals or lunatics and are best left alone. I may be full of parochial virtue but my effective horizon is the boundary of Pudlington Parva. The ethics of my village make up the code whereby good men live the world round. The reason for an action of a Pudlingtonian is the motive for that action anywhere in the world, be the action good or bad. I am an isolationist. A Hitler is only a village natural and we need pay no attention to him.

Since really effective knowledge of other peoples depends on frequency of contact with them, it is obvious that all nations are tarred with the brush of isolationism. But the thickness of the coat varies enormously. A small country that is not self-supporting has many trade contacts that directly or indirectly bring knowledge of foreigners and foreign ways to all its citizens. This knowledge is particularly extensive in countries with maritime interests. It is a small village in England that has no one who has been at sea and visited far-off lands. It is a large town in Russia that contains such a one. Even if travel were encouraged by the Kremlin the isolationism of Russia would remain. Its economy is largely internal and the bulk of its inhabitants live hundreds of miles from a frontier.

Therefore, even under the best auspices, it will always be difficult for Russians (or the inhabitants of any large continental area) to understand the point of view of foreigners. Many characteristic political features of a people may be shown to depend very largely on mental isolation.

This discussion of the feeling of 'reality' is highly important for an understanding of much social, as well as individual, behaviour. An idea dowered with much 'reality' is treated with the seriousness we accord to the properties of familiar objects. Our senses tell us that stones are hard and opaque, so we take account of those properties. We may know that they are composed of atoms but this information is not 'real' to us. We do not normally think of the properties of a stone as resulting from its atomic structure. But to the physicist these are the facts that matter. The greater our interest in any field of endeavour the greater is our feeling of 'reality' for the facts connected therewith. This is not only because of the emotional potentiality of events related to the interest but also because we tend mentally to rehearse the pertinent ideas. They become almost as 'real' to us as sensory experience. It follows from this that, no matter how we may in our philosophizing try to confine 'reality' to the objective and deny it to the subjective, any theory or abstraction that we constantly employ attains to a quasi-factual kind of reality. On the other hand, a bit of second-hand knowledge in which we are not interested has no such reality-feeling attached to it. In practice, therefore, we give a factual value to some 'thoughts' and deny it to some 'things'. Needless to say, this denial is neither verbal nor conscious; it is a denial by neglect.[1]

To put the matter another way, the psychological worlds in which two neighbours live may be different, or at least sufficiently different for it to be hard for them to understand each other on some topics of discussion. But, of course, the over-riding ethos of their community so controls their interest that, as to most problems, their views are mutually comprehensible. When, however, communication is attempted between representatives of markedly different cultures the worlds are so different that each is prone to think the other a lunatic or a knave—what is fact to one is negligible myth to the other. This is one of the most serious difficulties in the way of co-operation between communities sufficiently isolated to have developed a fundamentally different ethos. Further, such misunderstandings occur not only between nations but between different groups within the same country.

VII. Conclusion

An obvious cure for the types of misunderstanding with which we are concerned would be to increase contact. But equally obvious physical difficulties stand in the way of contacts between individuals sufficiently frequent and intimate to promote a sympathetic understanding of 'the other fellow's point of view'. We cannot all be ambassadors. Negotiators may agree but, when they report back to the bodies they represent, the agreement may not be ratified. One thinks here of American repudiation of the League of Nations. Politicians, especially, are rarely ambassadors. How could they be trained to study the prepossessions of the foreigner sympathetically? Presumably by the example of those who are most used to such a task. The automatic judgement of the layman who encounters a man with a totally different set of values is to write him off as a fool or a villain and to have nothing more to do with him. There are only two types of expert who have in their work to inhibit this tendency. These are the psychiatrist and the anthropologist.

The psychiatrist, if he is to understand his patient's thoughts and behaviour must

[1] An interesting parallel may be drawn between normal denial by neglect and the pathological condition of anosognosia.—(Ed.)

follow out for himself the consequences of a delusional or otherwise deranged attitude to life. He cannot just say, 'Oh, that's a silly idea, too silly to discuss', but must, so to speak, try to get inside his patient's skin. This demands both clinical experience and intuitive sympathy. In the same way, the anthropologist does not satisfy himself with collecting bows and arrows or photographing odd dances. He tries to find out what theories a primitive people hold about the surrounding world and man's place within it. In short, he tries to discover the psychological world of the primitive, knowing that, so long as he is ignorant of this world, he will not understand the people who live in it. The psychologist unfamiliar with the mental life of the primitive and the insane is a poor guide to better understanding of social relations in an advanced community. If the politician wants to acquire a technique for understanding the point of view of foreigners he should pay heed to the lessons of anthropology and psychiatry. 'But even so we shall have to struggle for an indefinite length of time with the difficulties which the intractable nature of man puts in the way of every kind of social community.'[1]

VIII. Summary

1. The concept of value, in its psychological aspects, is explained and it is pointed out that personality is in large part a matter of the relative values placed upon interests which are widely distributed. Although the relative strengths of interests cannot be expressed in numerical terms, it is possible to construct a scale of 'interest-values' applicable either to the individual or the group.

2. A number of criteria (other than self-ratings) may be employed to assess the relative importance of different interests in the individual. These comprise degree of compulsiveness, intensity of emotional reaction, response to conflicts of fealty, effect on moral judgement and level of psychic 'reality'. The criteria of 'interest value' are explained and illustrated in some detail and are shown to have some application to the study of differences between social groups.

3. The relation of belief to the sense of 'reality' is discussed at length and some implications of the latter for social psychology considered. It is pointed out that delusions resemble normal beliefs in possessing high 'reality value' but differ from them in so far as they lack collective sanction.

4. Gradations in the strength of the feeling of 'reality' are described and its significance for the mutual understanding of individuals and groups examined.

5. A plea is put forward for wider recognition of the psychiatric and anthropological approaches to the study of political problems in an advanced community.

Concluding note

I wish to express my appreciation and gratitude to the Master of Corpus Christi College, Cambridge, and Lady Spens, for so kindly granting me access to the manuscript of the late Dr MacCurdy's unfinished book and for their encouragement in preparing these extracts for publication. Among colleagues and friends who have helped me with advice and criticism I should like especially to thank Mr Martin Higham, of the University of Liverpool, and Mr John Langdon, of Balliol College, Oxford. I have also to record my appreciation of the services of Miss Julia Bunting, of Cambridge, whose skill in deciphering the late Dr MacCurdy's handwriting has made possible the preparation of these articles. Finally, I must express my indebtedness to the Editor of this *Journal* for his unfailing assistance and encouragement.

O. L. Zangwill

[1] Freud, *New Introductory Lectures on Psycho-Analysis* (1933), p. 232.

(*Manuscript received* 10 *May* 1950)

THE PLACE OF THEORY IN EXPERIMENTAL PSYCHOLOGY[1]

By ROBERT H. THOULESS

In choosing for the subject of this address the problem of theory in experimental psychology, I am not inviting the Society to abandon experiment, controlled observation, and mental measurement, and to return to the days when questions of theory were the main preoccupation of psychologists. Rather it is with the hope that if we can become clear as to what we are doing when we theorize, we may find ourselves able to spend less time in theoretical disputes which rest often on obscurities and misunderstandings. Nor am I claiming to be an authority on psychological theory. I feel very unclear on theoretical issues, and what I say is an attempt to think my way out of unclarity. If I condemn mistakes in theorizing, it is not because I am too clever to make mistakes myself. I think I have made them all; that is how I came to understand that they were mistakes.

None of us wants to go back to the days when the main interests of psychologists lay in the discussion of such problems as: 'Does green exist in the outside world or only in the mind?' or 'Is black a positive sensation?' Psychology was then primarily concerned with ways of talking about psychological facts, and little with these facts themselves or with any practical applications of its theories. A theoretical language is one of the tools of science. There is always a danger of interest in tools taking the place of interest in their use, whether these tools are words, or mathematical formulae, or pieces of apparatus. When experimental psychology came into being it provided a technique for enlarging the range of fact to be co-ordinated by its theories and, at the same time, diverted interest from the tools of psychological thought to their use in the prediction and control of behaviour.

It is arguable that this diversion of interest may have gone too far; that we have become so interested in practical achievements that we are content to work with a relatively inefficient set of conceptual tools and with no very clear idea of how to make them better. It remains true that our primary interest should remain practical and experimental; the tools must not again become an end. On the contrary, the fruit we may expect from a critical inquiry into the basis of psychological theorizing is that we may spend less time in discussing questions of theory and have correspondingly more time for experimenting. With an insufficiently clear idea of what we are doing in theorizing, we are in danger of wasting time in theoretical discussion of what to a more exact examination appear to be no problems at all but mere obscurities resulting from awkwardnesses in our use of language.

We do not now worry about whether green is in our minds or in the outside world, since we see this to be a question of linguistics masquerading as a question of fact. The linguistic question is whether we shall use the word 'green' for a property of experiences or of physical objects. Noticing that in everyday life it is used in both senses (though most commonly in the latter) we can avoid the danger of muddle in various ways, as, for example, by using a pair of adjectives such as 'phenomenal' and 'physical' to distinguish the two uses. As to black being a positive sensation, we should try to disentangle the linguistic

[1] Presidential Address to the British Psychological Society delivered at the Annual Meeting at Reading, 1 April 1950.

from the factual problem, and then settle the factual element in it by experiment if we find that it is not already settled.

We shall not become entangled in these problems, but we may find use for them as a warning to escape similar muddles in questions about the terms used in our current psychological controversies. We do still find psychologists concerning themselves with a very similar kind of problem when they discuss, for example, whether factors are mathematical parameters or unitary powers of the mind. I think there is a muddle here of the same kind as that about green being in the mind or in the outside world. It is not a question of fact but of language. Stated linguistically it is the question of whether to apply the word 'factor' to a system of mathematical parameters (the factor loadings) or to a unitary power of the mind which is taken to be indicated by this system of parameters. Stated in this way, there would be little disagreement about the answer. We have a right to use the word as we like, but it is obviously inconvenient to use the same word for two facts of such different order. My own preference is for reserving the word 'factor' for the system of parameters and using such a term as 'mental function' or 'faculty' for the mental characteristic which may in some way correspond to the factor. The important thing is that the term should not be used indiscriminately in both senses and allowed to create a pseudo-problem as to which kind of thing a factor really is.

The best way of avoiding unreal issues in psychology and of discussing those that do matter is to have a clear idea in our minds of what a scientific theory is and what is its purpose. We can express the matter briefly by saying that a scientific theory is a way of talking coupled with a system of rules that have the aim of enabling us to understand and co-ordinate some range of facts. Every science has what Ramsey has called its *primary system*, which in psychology includes experience and also the behaviour of organisms (1). We create a *secondary system* of psychological theory to bring order into this primary system and to make it, so far as we can, intelligible, controllable and predictable. The language of the secondary system contains terms (such as 'habit', 'goal-seeking', 'awareness', etc.) which do not occur in the language descriptive of the primary system. Not any language will serve the ends of the secondary system, since it is a characteristic of the language of a theoretical system that it leads to expectations as to what will or will not happen in the primary system. That is, a body of theory leads us to expect some things to happen in the primary system while it excludes the possibility of other things happening. It is the exclusions that are important for the testing of the secondary system. If what we observe is what theory led us to expect then the theory may be all right. On the other hand, it may not be, because we may later observe a new range of facts, the possibility of which was excluded by the theory. The testing of a theory is looking for facts which the theory excludes. If these are found, then the theory is inadequate and must be modified.

On the other hand, more than one theoretical language may lead to the same set of expectations. Then both are equally valid, although one may be more convenient than another because it is more economical when judged by the principle of parsimony. This principle does not mean that if two theoretical systems lead to the same expectations, the simpler is in some sense truer, only that it is more convenient. It is hardly necessary to add that if two theoretical formulations lead to different expectations, one has no ground for choosing the more parsimonious. The choice must then be made by considering which proves to be the best guide in the realm of the primary system.

The expectations aroused by the secondary system of a science with respect to its primary system are not all, of course, conveyed by the characters of the language used in the secondary system. They are also defined by certain rules (the fundamental *axioms* or *postulates* of the science and its *laws*) which also are a part of the secondary system. I think it is true, on the whole, to say that a highly developed science (such as physics) is more inclined to be dependent on its language and less on formulated rules for its expectations. For a different reason, a relatively undeveloped science, such as psychology, is likely to have relatively few verifiable postulates and laws, so that its expectations must be mainly defined by the choice of a theoretical language. There is an intermediate stage in which a large number of possible basic postulates or laws are in process of being tried out to discover whether their conformity with the facts of the primary system justifies their acceptance as accepted rules of the science. These candidates for the position of basic postulates or laws may be called *hypotheses*.

In the pre-scientific days of psychology, there was a set of postulates generally accepted as accounting for the whole of human behaviour. These were the laws of association and the law of the determination of human action by the expectation of pleasure. These look like promising foundations for a science of psychology but, though they fulfilled the requirement of delimiting expectations as to human behaviour, these expectations were in general not fulfilled by more exact observation and experiment. So we now regard them as laws covering only a small and relatively unimportant part of behaviour.

Hypotheses are, of course, used in experimental psychology, generally to define expectations within a limited range of fact. Both the stimulus-response hypothesis and the Gestalt hypothesis are too general and insufficiently precise to be taken as examples. They are rather general principles of theorizing which can be made the basis for hypotheses more precisely defining expectations. On the basis of the first of these, Clark Hull developed hypotheses for animal learning(2) and for human rote learning. Similarly, Köhler's hypothesis of diffusion of ions on the cerebral surface defined expectations in the field of figural after-effects. These are good examples of hypotheses defining precise expectations within a relatively narrow field which can therefore be easily tested by experiment.

It is necessary to emphasize the necessity for a useful hypothesis that it should be capable of being falsified by experiment, since sometimes the constructors of theories seem to work on the principle (more often implied than explicitly expressed), that it is a merit of a hypothesis that it is consistent with any primary facts whatever. Such complete flexibility may be given to a hypothesis in various ways. Thus the stimulus-response hypothesis may be made completely flexible if, whenever we discover an element of behaviour for which we can find no preceding stimulus as cause, we freely postulate an otherwise unknown 'internal stimulus'. Similarly, an indefinite degree of flexibility can be given to the theories of psycho-analysis and its derivatives by a sufficiently free use of the concept of compensation. A theory as to what will happen in a given situation may be put forward. If it happens it is regarded as confirmatory of the theory. If it does not happen, then a compensatory process is at work opposing the expected tendency. Thus any event may be explained by the same theory, and the theory by becoming indefinitely flexible loses all value as a guide to our expectations.

It does not, of course, follow that either internal stimuli or compensatory processes are illegitimate concepts. There may be other grounds for asserting them than the mere failure of our original expectations. They only become illegitimate when they are used in

such a way as to make the theories of which they are a part indefinitely flexible. The test of whether they are being used in a legitimate way is whether the propounder of the theory can give any possible observations which, if made, would falsify the theory.

Vagueness is also a common cause of a theory being unfalsifiable. Vaguely stated theories leading to no particular expectations rather than to others may often be found in the kind of psychological writing that Dr Oldham has referred to as 'dilettante psychology'. I find, for example, in a booklet professing to instruct its readers in psychology the following passage: 'The series of isolated thoughts with their potency set free, become motion thoughts, having the power to penetrate the mind of the eternal active spirit behind the universe.' One asks what conceivable observation would show this proposition to be false. I can think of no observation that would be relevant to its truth or falsity; if the author can, he gives no indication of what sort of observation it would be. This statement seems to me to be unfalsifiable through vagueness of indication. Such vagueness as a barrier to falsifiability is sometimes to be found in the writings of more academic psychologists when their self-criticism is somewhat relaxed—particularly when dealing with certain topics such as moral valuations, the uniqueness of personality, and some branches of social psychology and psychopathology.

One should perhaps also mention a third condition in which a theory becomes unfalsifiable—when, although one can state the observation which would falsify the theory, there is an invincible obstacle to making that observation. This is the case, for example, with any theory that the activity of animals is (or is not) accompanied by consciousness. Such theories are in the same situation as a theory about the visual appearance of the other face of the moon. They are far less common in modern psychology than they used to be but they are still occasionally found. Not infrequently it seems to be supposed that such propositions may safely be denied although they may not be asserted, and an author may assert that animals have no consciousness although he would condemn as unscientific a theory that they have consciousness.

Let us remind ourselves that the primary reason for requiring that a hypothesis should be falsifiable is that this is a necessary condition which must be fulfilled if we are to talk sense when we are theorizing. If we assert a theoretical proposition that is not falsifiable, and is therefore equally consistent with any fact whatever that might occur in the primary system, then we are not talking sense, and it is a waste of time to go on to argue whether the proposition is true or false. Also it is irrelevant to say that the proposition is confirmed by experience; a non-falsifiable proposition cannot be contradicted by any fact of experience. So when we meet with a theoretical assertion, let us always ask what possible fact would prove the theory to be false. If there is none, we need spend no further time in considering the theory.

If, however, a theoretical proposition when judged by this criterion turns out to be a sensible one, we next have to consider whether it is true or false; we must find out whether the expectations to which our theory points are or are not confirmed by experience. Here let us notice that although we may agree that a scientific hypothesis must be stated in a form that is falsifiable by some observation of fact, it does not follow that this observation of fact must necessarily be a simple one. This seems to be what is assumed in some current discussions of the problem, particularly by Clark Hull (2). It may well be that a valid and useful hypothesis can only be tested by a complicated and difficult act of judgement

involving observation of a wide range of facts. For example, there is no single observation that can falsify the Gestalt hypothesis of isomorphism or Freud's hypothesis of the determination of the symptoms of mental disorders by repression. Yet these may be legitimate scientific hypotheses because genuinely falsifiable, however difficult the process of falsification may be. Of course, there are advantages in working with simple hypotheses; it is better to be able to make an easy test than be forced to make a difficult one. But there is no reason for supposing that the hypotheses most fruitful for psychological understanding will be those easiest to test.

As has already been said, explicit postulates are less important in indicating the expectations of psychology than are the implications of the language used in its theory. Modern psychologists often start their books with a system of postulates, since they feel that only so can psychology be made into a real science. The postulates used are often, however, so vague as to be of little real use as a guide to expectations, and it is possible that the effort to express psychological theory in this way is premature. That many of our expectations as to psychological fact are implied by the choice of language in expressing our theories is often overlooked. It is also overlooked that we are free to choose our theoretical language, and that we are under the necessity to make this choice so that the implications of our language conform to the observed facts. The opposite procedure of examining our language and trying to deduce from it what the facts must be is an error of which the falsity is apparent when we realize that we have freedom to choose our theoretical language and that the facts to be expected will depend on this choice.

Although we are generally inclined to take a critical attitude towards explicit hypotheses, we are in some danger of treating as self-evident truths the implications of our theoretical language. This temptation must be resisted. The expectations aroused by our use of language are testing points for the adequacy of that language just as the expectations aroused by a hypothesis are testing points for the adequacy of the hypothesis. It may, for example, be urged against the legitimacy of the concept of free choice in volitional decision that in every situation in which a decision is made there are competing impulses of varying strength and that the strongest of these competing impulses wins. This is not, however, a demonstration that the conception of freedom of volitional choice is necessarily absurd, but that its possibility does not arise in a language system which refers action to the effect of competing impulses of different strengths. If we want to assert freedom of choice, we cannot use that language. If we want to test the adequacy of the language we must consider what it excludes (the concepts of moral blame and of responsibility as well as that of freedom of choice), and ask whether these exclusions are consistent with the language giving us adequate guidance as to our expectations in the field of human behaviour. This is clearly a case in which no simple observation can be enough for the decision; if it were there would be no difference of opinion about it. It is a matter depending on an act of judgement applied to a large field of human behaviour. It cannot be settled merely by taking for granted the implications of current psychological language.

In exactly the same way a number of expectations (such as that of the possibility or impossibility of our survival of bodily death) are directed by the use or non-use of the concept of a human 'soul' or 'spirit'. When, until fairly recent times, mental events were spoken of as events happening to a 'mind' or 'soul' it was not unnatural to think of this as something which could carry on a life of its own after the body was dead. Indeed to talk of a 'body' and of a 'soul' controlling that body implied that they were two things either

of which could exist apart from the other. But this simply meant that one used a language that predisposed one to think of the soul as surviving bodily death; it was not a rational ground for supposing that there really was survival.

The converse, however, is equally true. With the rise of a physiological psychology during the last fifty years, psychologists have rejected such concepts as 'mind', 'soul' or 'spirit', and adopted a theoretical language in which mental processes are not spoken of as activities of a soul, but as aspects of the physical activity of a material nervous system. Obviously this language system arouses the expectation of our complete extinction at bodily death. There are other expectations more easily tested, such as the impossibility of the conveyance of ideas from one person to another without material means of communication. Non-survival of death is sometimes spoken of as if it were an assured inference from modern psychological science. It is, of course, not really that; it is an expectation aroused by the fact that we have stopped talking a language in which survival is a reasonable expectation. The crucial test of the language will arrive when we die. From conversation with my psychological colleagues I gather that most of them would be astonished if they found that they themselves went on after their bodies were dead. They should not be very greatly surprised if they realize the purely linguistic basis of their expectation of extinction. They should simply say: 'So our theoretical language was inadequate after all!' Their situation would be no different from that of the theoretical physicists when Einstein proved the necessity for the change over from the language of Newtonian mechanics to that of relativity.

That crucial test is one for which we must wait; there are other tests which we can apply without waiting. There may be, some day, adequate evidence for the survival of other people from mediumistic seances, although I do not think that there is yet. On the other hand, there is incontrovertible evidence of the possibility of the conveyance of ideas from one mind to another without material means of communication. Few facts in psychology have behind them the weight of evidence that there is for extra-sensory perception, and that is just as critical a test for the adequacy of the language under consideration as anything we may experience after our death(3). It shows that while the theoretical system which excludes the soul concept may co-ordinate a large range of the facts of the primary system, it fails to co-ordinate all of them. Again the fact of extra-sensory perception should not cause more than mild surprise to the psychologist who realizes the linguistic nature of his reason for not expecting it to happen.

I have said earlier that we are liable to waste time on unreal issues if we fail to distinguish between questions of language and questions of fact. There is a typical pseudo-problem of this sort, of which an example of current interest is the problem of the status of the factors determined by the process of multiple factor analysis. This is a particular case of the more general question of the 'reality' of the objects referred to in the theoretical language of any science: 'Are electrons real or are they mere logical fictions?', 'Is memory a real unitary function of the mind?' or 'Is the mind itself real?'. I suggest that the questions are wrongly stated. We are not concerned with an object and the question of whether it is real, but with a word and the question of whether it plays a useful part in our conceptual system, that is, whether that word helps to co-ordinate the facts of our primary system.

The question occurs generally in connexion with the nouns of our secondary system. When we use nouns we are inclined also to inquire into the reality of the objects that they

represent. To avoid this kind of question, some methodologists say that we must discard nouns from our theoretical system. This extreme act of self-denial is, I think, unduly hampering, unnecessary, and not really serviceable. It is hampering because it is difficult to talk without the use of nouns. It is unnecessary because nouns are harmless elements of the secondary system if we resist the temptation to discuss the reality of the objects we are inclined to think of them as representing. It is unserviceable because it is not able in itself to achieve the desired end of avoidance of discussion of the pseudo-problem of 'reality'; we can also become entangled in parallel discussions about other parts of speech, such as verbs or adjectives. We can eliminate the word 'memory' from our theoretical vocabulary and talk only of 'remembering'. There is, however, nothing to prevent us from discussing whether the activity represented by the word 'remembering' is a 'real' activity, except our determination not to enmesh ourselves in that sort of problem. If we have made that determination and have strength of mind to keep it, then, I think, we can safely use the noun 'memory' also.

That there was a problem about the meaning of nouns in psychological theory was first recognized in connexion with the system of faculty concepts—memory, will, imagination, etc. There were, of course, good grounds for questioning the idea of real entities indicated by these nouns and it might also reasonably be asked whether the mode of classification of mental processes implied by these nouns was the best one for theoretical purposes. More commonly the concept of faculties is nowadays criticized on the far less reasonable ground that the mind is an undivided whole and that these words imply that it can be divided into parts. But in other fields of thought we unhesitatingly use nouns to stand for elements in unitary wholes without supposing ourselves to imply any division of those wholes. We talk of the eyes, nose and mouth without forgetting that the face is a unitary whole which cannot be divided without losing its character of being a face. We talk of the mass and extension of a body without being held to imply that it could be divided into two parts of which one was extended but of no mass while the other was massive but without extension. In ordinary speech and in other sciences we use terms for elements or aspects of a unitary whole without being held to imply anything whatever about the divisibility of that whole or the separate existence of the parts, and I see no reason why we should not allow ourselves the same liberty in psychology.

The fear of the concept of 'faculties' or of any related concept is, I think, an example of the danger of showing too great respect for the prohibitions imposed by past controversies. I find, for example, in the article on 'Faculty Psychology' in the current edition of the *Encyclopaedia Britannica*, the following passage: 'Although the faculty psychology has been frequently derided[1] from the days of Locke onwards, it is not entirely dead, for it still seems to haunt some of the latest books on psychology in the guise of "abilities".' And why not? Modern psychologists might reject the concept of 'abilities' as not of use in their system of theory, but it must be for a better reason than the derision which has been meted out to a related concept in the past. If concepts have been used by our grandfathers and rejected by our fathers (even if they rejected them for sound reasons), yet new ranges of fact in the primary field may again require their use.

There is an obvious danger to clarity of theoretical thinking in allowing any past polemic to reduce a concept to such a level of deadness that it becomes a mere term of abuse. This has clearly happened to the word 'faculty' since it has, for same time, been

[1] 'Derived' is the word used in the article but this is obviously a misprint for 'derided'.

considered a sufficient condemnation of any psychological theory to say that its concepts (group factors, instincts, etc.) are merely resuscitated mental faculties, thus securing the rejection of the theory with maximum economy of mental effort.

This economy can also be secured by using similar abusive terms from other past controversies. A theory can be condemned by calling its concepts 'atomistic', 'static' or 'metaphysical'. Similarly, we can commend a theory without expenditure of mental effort by applying to its concepts such good terms as 'functional', 'dynamic', 'whole', or 'biological'. In some contexts all these terms have been used with precise meaning, but they are inclined in current controversies to become merely the vehicles of emotional attitudes instead of means of intellectual thought. The emotional use of language should have no place in scientific thinking.

Let us return to the question of the usefulness of discussing whether there are real objects of the nouns used in our theoretical system, such, for example, as abilities or personality traits. Why should this not be a profitable question to discuss? Of course, sensible questions may be asked about the reality of the things denoted by nouns. One may ask whether the mongoose kept in a basket by a delirium tremens patient is real or imaginary; one may ask whether an image formed by a lens is real or virtual. But the question as to whether a factor or a faculty is real is not a question of this kind. It is rather a question related to the pseudo-problem of the idealist-realist controversy 'Is there a real physical world?'. This we condemn as a pseudo-problem because there are no verifiable differences in our expectations if we answer it one way or the other. We see physical objects about us. There are a lot of further questions we can ask about them: 'Could we touch them if we didn't see them?', 'Could they still affect a photographic plate if no one was looking at them?', 'Would they show changes during a time when they were not under observation?'. To all such further questions we know the answers, and the subjective idealist who denies the reality of physical objects and the realist who asserts it have no disagreement about these answers. So none of the verifiable questions that can be asked about the tree is the mark of the 'reality' under discussion. This is the characteristic of the pseudo-problem.

When we discuss the 'reality' of 'memory' or of 'the general intellectual factor' or of 'the soul', we are discussing a parallel question. It is different in this respect that we do not know the answer to a lot of the further questions that may be asked. We can ask, for example, whether the general intellectual factor is inherited as a Mendelian unit, whether it is localized in some part of the brain, etc. Whenever one of these questions is answered the connotation of the term 'general intellectual factor' will be correspondingly enriched, but at no point of this further enrichment will it be properly expressed by saying that now we have discovered that the general intellectual factor is real. The general intellectual factor is a concept whose connotation is those known facts which we use the term to co-ordinate. Always there may be new facts to be co-ordinated by the term, but there is no single new fact and no combination of facts which can properly lead us to say that the object of the concept is now known to be real.

In saying this, I am not intending to endorse the common criticism of factor analysis that its products are not 'psychological realities', for this is generally made with the implication that the critics have some other method of determining psychological realities. For the purpose for which it is designed, factor analysis is, I think, the best method at our disposal. If we want to find out, for example, whether the classification of mental tests

into those measuring ability in a verbal direction and those measuring it in a perceptual direction is of theoretical value, there is no better method than to take a group of both kinds of test and perform a factor analysis on their intercorrelations. If this process shows a separation between the two types of tests, then a conceptual term distinguishing them is a serviceable one. No other method can do more. We can also find out that the term used is one which makes a distinction in ordinary speech or a distinction which has been made in psychological theory apart from factor analysis, but this is simply to enrich its connotation, not to show that it corresponds to a 'psychological reality'.

If we abandon the pursuit of 'psychological reality' we need no longer be disturbed by the fact that a factor analysis may give us not one but many solutions of the problem it is meant to solve. Early in the factor controversy, Prof. Thomson pointed out that a relation between test scores which could be referred to the operation of a general factor common to all performances could also be referred to a set of overlapping group factors common to some of the performances without any factor common to all(4). This point occurs in a more general form in multiple factor analysis, where rotation of axes gives alternative equivalent solutions. In all such cases, the solutions are equivalent in the sense that they lead to identical expectations. In that case, there is no sense in asking which is right and which wrong unless we hold that some concepts stand for psychological realities while others do not. If that idea is rejected, all solutions leading to the same expectations must be held to be equally good solutions; some may be convenient forms of expression for expressing some expectations, while others are convenient for other expectations.

Alternative equivalent conceptual formulations are familiar in other sciences. The fact that two bodies approach each other may be expressed by saying that they are acted on by a gravitational force proportional to their mass, or by saying that they move in a way reducing their potential energy to a minimum or by saying that they travel along a geodesic. Similarly, the behaviour of light at a refracting surface may be expressed by saying that the ray is bent towards the line normal to the surface when it enters a medium of greater optical density or that it travels along the quickest route. The physicist does not dispute which of these formulations expresses physical reality. He accepts them as different ways of saying the same thing. So, I think, are the alternative solutions of a factor problem.

The last question I want to discuss is whether we should expect to be able to co-ordinate the whole of our primary system in psychology by means of a single secondary system of theory or whether it may be necessary to employ several. Most of us do, in fact, adopt a somewhat eclectic attitude towards theoretical systems making a choice between a behaviouristic, a field system or a voluntaristic system in accordance with the needs of our problem. Those who are consistently mechanistic in their work are inclined to use concepts excluded by mechanism in their judgements of human behaviour outside the laboratory, assigning moral valuations and distinguishing between action under uncontrollable and controllable impulses. In general, we are somewhat ashamed of this eclecticism, and suppose that it is only a transitory imperfection in our science due to its undeveloped state. There have, of course, been many attempts to bring all psychological facts under one all-embracing theoretical system, but such success as has been attained has been at the cost of sacrificing a good many of the facts. The history of such systems as associationism and behaviourism shows how willing theorists have been to make this sacrifice when the overriding demands of a neat unitary theoretical system have demanded it.

We should, however, also consider the possibility that the sacrifice may not be necessary, that the use of more than one theoretical system is not a passing imperfection but a permanent necessity for full co-ordination of our facts. This seems to be the case in theoretical physics. I understand from Prof. Heisenberg that it is necessary to use four theoretical systems which contain theoretical languages and sets of postulates that are not mutually deducible from each other. These are the systems of Newtonian mechanics, of relativity theory, of quantum theory, and of Gibbs's thermodynamics. These he calls 'closed systems'. While not deducible from each other, their expectations are not contradictory to one another. The expectations with respect to any set of physical facts may require reference to more than one of these closed systems.

It seems likely that the situation is the same in psychology. Instead of regarding behaviourism, the Freudian system, and a voluntaristic theory, as alternative systems of explanations of behaviour it may be more correct to consider them as closed systems of which more than one may be necessary to explain any behaviour fact. To know what will happen when a particular man is insulted we must consider, for example, not only what response has been conditioned to that stimulus, but also what processes it activates on the unconscious level, and, in addition, the possibility that he may decide to do something different from what these determinants of behaviour would lead us to expect.

If theoretical physics needs four closed systems, it seems unlikely that psychology will do with less. I suppose the systems we are working with now would have to include at least: reflexology (or behaviourism), Gestalt (or field) psychology, voluntaristic psychology, and associationism. I think we must add parapsychology as another theoretical system whose relation to normal sensory psychology may be that described by Heisenberg as enclosure, i.e. all obtaining of information through the senses may be a special case of the more general principles of obtaining knowledge embodied in parapsychological theory, just as the language and laws of Newtonian mechanics are a special case of the more general language and laws of relativity theory.

It is obvious that we have no exact methods of quantitative verification such as have enabled physicists to determine how many theoretical systems are adequate for their facts. The number required in psychology may be greater or less than the list I have given. What is important is that we should keep our minds open to the possibility of more than one theoretical system being necessary in psychology. This will have two consequences. First, we shall be more willing to consider a new range of facts which does not fit well into our existing systems of thought. Secondly, we shall be more ready to consider the usefulness of a theoretical system which plainly does not cover all the facts. Newtonian mechanics is not a discarded theoretical system in physics because it does not cover all the facts; it is still a useful system of explanation within the range of facts it does cover. So also in psychology such systems as the stimulus-response theory may have a continued usefulness as providing understanding in a limited field. They may only become dangers to psychology when extended beyond that limited field. It may be of value to the science of psychology to construct a relatively simple theoretical model which co-ordinates some range of facts, although we do not claim for this model universal applicability. On the other hand, it may be a disservice to psychology to insist that all theoretical formulations must be adequate to the whole range of facts. The colour-solid, for example, is a useful theoretical construct although it does not take into account what Katz has called the 'modes of appearances of colours', or the social aspects of colour preferences.

I will close with a quotation from the discourses of Gotama Buddha. A persistent theme of his remarks is in some such words as these: 'These questions, O brethren, are the questions which you ought not to discuss, and these other questions O brethren are the questions which you ought to discuss.' That I think sums up the value of a systematic methodology to guide our psychological theorizing. Questions of linguistics masquerading as questions of fact, questions as to the reality of the 'objects' of our theoretical concepts, these are questions which we ought not to discuss. Questions of the adequacy of our theoretical formulations to cover the facts of our primary system, questions of the defining of our concepts to make them serviceable to the ends of theoretical classification, questions of the limitation in field of our secondary systems; these are some of the questions which we ought to discuss.

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(*Manuscript received* 23 *May* 1950)

THE INTEGRATION OF PSYCHOLOGY WITH MORAL VALUES

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I. The place of values in psychology

Once upon a time, when psychologists were cloistered in laboratories, and had no knowledge either of the total personality or social groups, only an occasional, eccentric psychologist ever studied ethics. But this age of innocence is gone; the gates of Eden are closed, and the social psychologist has been forced to stray where right and wrong present a persistent problem which must receive his professional attention.

Many of us are anxiously waiting to see whether this new wanderer among the social problems of our time will succeed in illuminating political life with the searching reason and calm integrity of science, or whether the brute passions of the market-place will instead be released to pollute the laboratory. The latter is a real danger if this wanderer continues also to be a somnambulist, unawake to the dawning need for the psychologist to make himself a student of values.

A previous article (3), which raised a challenge to the misuse of values in social science, has been misunderstood by some to mean that the present writer favours denying the introduction of human values into scientific questions (5). Actually the whole sense of that article was that we should give far *more* attention to the introduction of values,[1] but in the right place (3). I feel as if the waiter had assumed that I do not want soup when all I said was that I do not like soup in my coffee. If dirt is properly defined as 'material in the wrong place' then, I repeat, many current writings by soi-disant 'social scientists' need badly to be sent to the laundry.

II. The duality of principles in applied or 'action' research

Since I propose to go beyond the above-mentioned (3) article on 'Ethics and the social sciences' it is desirable that I summarize first the propositions made therein. They are:

(1) That in any recommendation by a social scientist two threads of reasoning are interwoven. One concerns a series of causes and effects linked by knowledge of scientific principles. This is a matter simply of technical prediction that if A is done B will probably follow. The second train of thought is a series of moral judgements, that if Y is an ultimately desirable ethical or human goal, then X is a course of action that is socially recommendable. This chain may utilize the knowledge about cause and effect found in the first chain, but it is an independent argument.

(2) As trained at present the psychologist is fitted only to give *ex cathedra* public statements on the first of these principles—the technical ones. On ethical matters he has no more authority than any other man in the street.

(3) When we search for the ethical and human values which need to be brought into conjunction with the technical scientific principles we find them either in (*a*) the intuitive and dogmatic beliefs presented by religions, political ideals, and great literature; or (*b*) in the rational forms propounded by philosophers and other students of ethics.

[1] Thus I agree, for example, with Karl Marx that 'Hitherto philosophers (scientists) have sought to understand the world; henceforth they must seek to change it', though I cannot agree with the shallow, materialistic and uncharitable values by which Marx would have changed it.

(4) That social scientists should avoid public odium and the suspicion that they are betraying their scientific trust to particular political or even commercial ends. This they can only do by always explicitly recognizing in their writings the duality of scientific and ethical principles, indicating by what combination they arrive at the recommendation offered. The dictates of conscience require this, however, no more than do the demands of common sense. For by this separation we ensure that if one be found faulty by later research the whole need not be thrown away.

Had I not encountered the argument I should scarcely think it necessary to add that the reply, 'I recommend this for the sake of human progress', is no substitute for the above standards. It is many years since G. K. Chesterton, in one of his incisive essays(4), reminded those who needed it that 'No man has a right to talk about progress unless he has a creed'.

When a social scientist makes recommendations for progress it is surely not too much to ask that he state clearly what human values he regards as most important.[1] At present we shall naturally have to accept these in a far less critical spirit than the purely technical principles. Some writers, like the one just mentioned, will point to the Catholic Church, others to the values of Protestantism; some will point to Marxian philosophy (being careful, I hope, to deny party membership, unless they happen to be Russian writers!), while others instead of pointing to ready made, mass-produced value systems may define precisely values of their own. In speaking of precision I question, for example, whether a social scientist can to-day use the unqualified term 'democratic' in a scientific argument, for there is American democracy, French democracy, British democracy and, apparently, even Russian democracy; and although it takes little ethical training to see that each has different values it cannot be assumed that the reader knows to which the casual writer refers. In addition to the more profound, widely ramifying value systems there exist in some studies more immediate and urgent values which should also be stated. Thus some so-called 'prejudice' studies are concerned partly with universal values and partly with raising the status of particular groups, relative to all others; some psychotherapeutic theories insert values regarding the release of inhibitions on sex or aggression; and social psychologists' recommendations on war seem implicitly to assume that the reduction of human stress and conflict takes precedence over values which some religions would place prior to this. Particular examples of these hidden values will be raised in the next section. Here a few are cited simply to show that from a scientific point of view it does not matter whether we agree or disagree with given values, but that it does produce a very unscientific confusion unless they are stated.

As the above illustrations show, the ethical value systems offered may legitimately vary widely in generality, organization and popular acceptance; but if they are honestly and exactly stated this will never produce the hostility or the loss of repute of social science among legislators and research foundations that is likely to result if our less responsible members attempt to foist their personal value predilections upon the public in the guise of scientific conclusions.

[1] As Glass(8) reminds us 'progress' in some countries means suppressing scientific evidence and free speech. He reminds us of the courageous stand of the American geneticist Muller who, when politically attacked while in Russia, 'reaffirmed the Geneticists' Manifesto in 1939 and asserted that loss of the right to establish truth by scientific methods, though in the name of a program for the betterment of man's social and economic welfare, is too great a price to pay'. As shown below, the same appeal for suppression of inconvenient facts is being made by some American 'scientists'.

III. EXAMPLES OF THE ILLEGITIMATE INTRUSION OF VALUES

have already indicated that two of the simpler propaganda practices that need to be lloried are the gratuitous use of the term 'prejudice' and the use of emotionally weighted ıt undefined terms like 'undemocratic' (or the use, as a term of opprobrium, of such ɔels as 'capitalist' in Russia, or 'democratic' in Spain). In regard to the former, it is ear that a writer sometimes means only an *antipathy* to some idea or group, and would ve to say that Christ had prejudices against moneylenders and Pharisees, though in fact ese antipathies fitted with relentless logic and clarity into a Christian value system.

The basic objection to some current uses of the term 'prejudice' is a very simple one: that ey throw away the meaning of a good specialized word, by making it merely synonymous th attitude or sentiment. A prejudice is properly defined as a sentiment or attitude *ere the facts, or the values, or the logic connecting facts and values, are demonstrably false.* ıe use of prejudice in any other sense is commonly itself an act of prejudice. This brings to the second objection, to some current psychological uses of the term: that they tend perpetuate an exchange of aggression. For by calling views with which they disagree rejudices' the alleged 'investigators' are merely following a polite form of the bar-room iral which begins with 'You're a liar' and 'You're another'—which is scarcely calculated bring the issues within the calm of science.

Before using the word prejudice with such fine abandon in scientific work we should at ast investigate the possibility that many differences of opinion among men do not arise ɔm failures of reasoning or disagreements about fact, but from fundamental differences value systems, originating either in real differences of temperament or in historical acci-ents of cultural background. That values may be rooted in temperament was convincingly gued by William James in well-known passages, and the recent factorizations of attitudes ɔng with temperament measures strongly support it. A plea for the toleration of such titudes as 'differences' rather than 'prejudices' has been cogently made by Ichheiser (11), ıo points out that 'prejudice' studies to-day 'often reveal more about the bias of the ırticular social scientist than about the attitudes of the common man who is allegedly the ɔject of investigation'. This woefully unscientific bigotry which Ichheiser exposes lacks e ability to ask basic questions which even the poet knows, as when Browning more lerantly asks:

> Now, who shall arbitrate?
> Ten men love what I hate,
> Shun what I follow, slight what I receive.

Of course, no one objects to the psychologist, when the blast of war blows in his ears and has to mount his steed in Washington or London, using this or any other device of ychological warfare. Then let him ambush his listener with hidden values and use whatever abolical skill he can summon in the cause of propaganda. But if he is a good scientist he ll do this only as his duty, regretting these habits of distorting truth as much as a biological ırfare expert dislikes releasing dangerous bacteria. And he will always recognize that e one place where propaganda cannot be tolerated is in scientific work, either in discus-ɔns among scientists or in popularization of science for the general public. The pages of me of our journals are already sufficiently obscure, without devastating them further th propagandistic blitzkriegs and undeclared political or religious wars.

All the fields of applied psychology—political, industrial, educational and clinical—ɛld examples of values intruded in the guise of scientific principles, but it is perhaps only

in the socio-economic and the clinical areas that this unholy masquerade has really gone too far. In the former, as my previous article pointed out(3), even the politicians have noticed what some 'social scientists' are up to, and the matter has since been commented upon by Ex-President Hoover, in his speech at Stanford University where he mentioned their habit of 'manipulation of words, phrases and slogans to convey new meanings different from those we have long understood' and asked, as a remedy, for 'the qualities of self-restraint, of integrity, of conscience and courage'.

Examples directly in clinical practice (at least in 'directive therapy') are too numerous to mention, but a recent instance from a study of sexual behaviour may illustrate the general nature of the intrusions. Terman, in his review(17) of Kinsey's report, points out that 'Notwithstanding Kinsey's frequent reiteration that his job is to report facts rather than evaluations, it has been possible to quote numerous passages in which recklessly worded and slanted evaluations are expressed, the slanting being often in the direction of implied preference for uninhibited sexual activity.'

Most examples of dragging in values under the cloak of science seem due either to *naïveté* or to slyness, but occasionally it occurs as a brazen attack on scientific standards, as in the writings of Lysenko(15), who tells us that 'the foundation principles of Mendelism-Morganism are false. They are...an example of idealism...the Mendel-Morganists of the Soviet Union, though literally fully sharing the principles of Mendelism-Morganism, often conceal them shamefacedly....' This last does not appear so odd when the President of the Soviet Academy proceeds to list the unfortunate professors who have mentioned Mendel or Morgan and to add 'We...must squarely admit that we have hitherto proved unable to make the most of the splendid possibilities created in our country by the Communist Party and the government for the complete exposure of the Morganist...', and continues later to state that good Soviet scientists must have 'come to realize that Morganist views are utterly alien to the world outlook of Soviet people. In this light the position of Academician P. M. Zhukovsky does not look so nice, seeing that he advises young biologists, etc.' One does not need to be a politician to realize that Academician Zhukovsky's position is not only not 'nice' but positively not safe! One wonders how many of the less responsible social scientists in this country who have surreptitiously used science for propaganda realize that the logical end, if they were allowed to continue, would be that the pretty axes they have tried to grind might be hoisted over their own necks?[1] The ugliness of Lysenkoism can only be kept out of science here by a vigilant attention from the very beginning to the task of keeping values (political, religious, or whatever) in an explicit, proper, and defined detachment from scientific conclusions.

IV. The peril of action research

Among recent attempts to employ for cheaper socio-political purposes the dearly bought integrity and prestige of science and research is that strange movement which some of its devotees have been calling 'action research'. It happens that I have recently debated this

[1] At the moment of writing these lines the public press reports (in syndicated column of Dorothy Thomson) 'Four Russian physicists have been disgraced and disappeared for saying one cannot predict the behavior of electrons, this being anathematized as the "heresy of the bourgeoise Copenhagen school of physics."' Meanwhile, Dr Glass(8) reminds us that the above political persecution of geneticists is an old story, for, after two political attacks on scientists, 'in 1939, a third genetics conference met, and the charges against the "classical" geneticists were repeated. Nikolai Vavilov was made a special target for attack, and after the conference this world-renowned scientist was relieved of his posts and "disappeared" in Siberia.'

atter—not with one of its less reputable representatives but with a person of the scienti- standing of Dr Cook, who recently received the Bernay's award of $1000, under the spices of the A.P.A., for a particular action research. Dr Cook, who showed in this study inter-group attitudes that criticisms of groups by out-group members could be signifi- ntly suppressed by immediate criticism of the speakers, has objected to my criticisms of tion research. He quotes with disapproval my judgement that the concept of action search is 'ill-considered' and 'a meretricious appeal for action on scanty, even perfunc- ry research, that would quickly lead to a wholesale prostitution of social science'(3). ır debate[1] has been beneficial in clarifying and restricting the points at issue, but does ot seem to me to require any great change of my evaluation specifically of action research, r reasons to be given in this section.

All scientific research, Dr Cook points out, is liable to reach somewhat distorted con- usions if those who conduct it have strong desires to reach certain conclusions. (The nscious and unconscious processes of selection of subjects, conditions and steps of asoning by which this distortion occurs need not concern us here.) Against this, he says, e have the usual checks and safeguards found in scientific canons and in the repetition experiment by others, so that the additional safeguard which I advocate—a statement what the scientist wants to believe—is superfluous.

I agree with this formulation up to the last statement and with the proviso that, as Marquis has pointed out, it omits reference to the major part of the problem which I troduced. I am not so concerned about the distortion of scientific findings themselves, which, as Dr Cook says, we have the usual checks. *It is in the social scientist's use of se scientific findings as a basis for specific social recommendations that the danger of olesale misapplication and misconstruction arises.*

Parenthetically, however, it may be pointed out that even for the evaluation of purely entific conclusions some knowledge of the views and reliability and capability of the rson conducting the research is not to be despised. In the end we always apply the test scientific canons and repeated experiment, but in the meantime, being human and short time, we all like to know who did the research and we shape our tentative conclusions its probable reliability and fertility on our experience of the person in question and our owledge of what he would like to believe.

Dr Cook has sought to avoid the difficulty of the application of findings by taking an stance—that of preventing riots among whites and negroes compelled to live in the same using project—in which he restricts his task simply to finding out what conditions may crease or increase the friction, leaving to the authorities the responsibility of making iat use of the data they require. Other speakers for action research, however, have sagreed with this, saying that laymen make very poor and even misguided use of the in- rmation supplied them, so that it is desirable for the social scientist to prescribe through- t not merely the scientific facts but also the use to be made of them.

As a psychologist engaged in basic research remote from any clamour for immediate plication, I can sympathize with Dr Cook in the restriction of thinking which the brute mands of immediate action research must entail. I happen to agree with the endeavours the group using his data, but this is only one possible value framework, and several iers could be legitimately stated and examined which would lead to different 'action search' questions being asked. For example, in the solution of the problem he brings up

[1] Annual meeting of the A.P.A., Denver, 11 September 1949.

some psychologists, e.g. McDougall, would first want to raise more basic questions, such as whether it is ethical to compel people to live with neighbours they do not enjoy instead of with those they do, and even a less radical inquiry might question why this dilemma should be forced on the recipients of subsidized housing, merely because they are poor, while the more well-to-do are permitted to buy themselves into whatever choice of neighbours they prefer.

The same disinclination of 'action researchers' to face wider issues and social obligations appears in their serene disregard of what happens to the aggressions deflected from the particular interests they are paid to defend. As psychologists we know that in nine cases out of ten the suppression of aggressions by force results in a heightened aggression being directed in some new, internal or external direction. The cumulative irritation which persons of adult intelligence experience from radio commercials, may, for example, be deflected by the devices of the sponsor's psychological mercenaries from its true object, but may it not find vent in, for example, an irrational degree of hostility to out-groups, worsening the international situation? Soldiers in the front line have been known to express a half-ashamed satisfaction when their compatriots along the line received a barrage which might otherwise have fallen upon themselves. But there seems to be not even the glimmer of a conscience—still less of any true research interest—among 'action researchers' concerning the effects upon the whole society of the ricochets from their own concrete hide-outs.[1]

In short, it seems questionable whether the social scientist can or should escape from the problem of having to make a thorough moral examination of the use being made of his results or of the set-up in which he is asked to answer a specific question. The allied courts examining Nazis have been full of defendants pleading that they acted only as professional advisers and had no responsibility for the applications made, and we know that shockingly inhuman acts were committed, for example, by medical men taking this standpoint.

In the U.S.A. we may thank heaven that we are not likely to have to encounter such consequences of failure of this principle, but it is none the less urgent that we get the principle clear. In this country the powers that call the tune for the application of scientific research are more frequently financial than political. In so far as these set up the framework within which applied or action research is done they can (*a*) ensure (in the case of scientists not interested in what use is made of their findings) that only certain applications, in keeping with the interests of the financial sources, are made; and (*b*) decide the areas in which research shall be done (except for that minority of scientists more interested in doing research than in being paid for it).

[1] Another respect in which 'action researchers' show a dangerous moral confusion and failure to recognize the consequences of their ideas is in the argument that facts which the 'general public' might misunderstand should be withheld. For example, it is said that the psychologists' finding that negroes perform at a lower mean level than whites on intelligence tests had best not be mentioned, lest someone 'misuse' it. It is pointed out that the combination of a wrong attitude with a wrong fact may in this instance produce the right action! (See also the similar observation by Klineberg on Boas in my previous article (3).) On the contrary, it seems to me that the sooner the right facts are known the sooner the general public will be forced to correct wrong attitudes (in this case the attitude that a slight difference in average intelligence test performance justifies exclusion from the community). It is in this beneficial way that the scientific, factual thinking about physics which has produced the atom bomb is now bound to produce correct attitudes of scientific, factual thinking about social problems in place of pre-scientific modes of thinking. Truth in one part of a field makes untruth in another part untenable. But the 'action researchers' are apparently more concerned about some petty local gain than about the propagation of scientific objectivity and illumination.

Dr Lentz has raised the question of how far it is morally permissible to buy up good research men from important research to draft them to other research of interest to the sponsor but possibly of less value to the well being of the whole community and certainly of less value to science. For example, if some of the psychologists now engaged in improving the advertising of brands of soap could be recruited to the all-too-small band of workers in basic research on group behaviour and international tensions there is no question that the community as a whole would be much better served. To that important problem of organizing a distribution of research funds (by financial control) which will be the best for science, the present issue is only tangential, and we shall not pursue the problem here. However, it is relevant to point out that the present distribution of funds gives far too high a ratio of applied (including 'action') research to basic research for any harmonious and effective scientific advance. The tail is far too big for the dog. At present the only saving feature in the situation is that men of the finest research calibre are still too devoted to the intellectual and moral appeal of basic research to be seduced by the glittering prizes of commercial, applied research. Nevertheless, some good intellects are lost.

V. The possibility of a scientific ethics

The above section has tried to reveal the very practical importance of psychologists, and scientists in general, being explicitly aware of the social and moral values which support their research and utilize its results. It is necessary now to return to the question of the practicability of *stating* these values when the scientist *has* become aware of them. For it is objected by some that the scientific standard requiring an explicit duality of presentation of scientific and ethical principles is impractical. It is said, first, that few writers in social science are sufficiently interested in the standards and goals of pure science; and secondly, that the demand for an explicit statement of ethical values is asking more than we are trained to give.

The first of these scepticisms seems to me as cynically unrealistic as Hitler's or Mussolini's illusion in 1939 that the democracies would not fight. Interest in clarifying the relation of ethics to social science has been manifested increasingly in the last few years, not only by such discussions as the present one in psychology, but, for example, in economics by Schumpeter's *Science and Ideology*, in sociology by Lynd's *Knowledge for What?* (14), as well as by the writings of Baker (1), Haldane (9), and Crowther (7), by Max Weber's chapter on 'Objectivity' in *The Methodology of the Social Sciences* (19) and Waddington's *Science and Ethics* (18). All of these recognize that science has an increasingly important role in social reform, while jealously insisting that it must retain the standards of science. The fact is that scientific men can always be depended upon to rally and fight to the last ditch against any attempt to subordinate the scientific ideal of truth for its own sake to any commercial, political, or religious tyranny. For truth, and the toleration necessary for finding it, are the only values intrinsic to the scientific spirit, and blasphemy against this spirit is the one sin that no true scientist can forgive.

Against the second charge of impracticality, on the other hand, I can raise only the uncertain hope that psychologists, or, at least social psychologists, will study ethics as part of their training and that, moreover, the day is not far distant when ethics will be successfully shifted from an intuitive to a scientific basis. For although some of the keenest scientists realistically recognize that it may now be necessary to be content with 'revealed'

religious ethics, they are also uneasily aware, by temperament and training, that forcing the new wine of science into the old bottles of dogmatic religion can only be regarded as a temporary solution. The development of a scientific ethics, such as would permit a nobler integration of science and values, is nevertheless a tremendous undertaking. The hope that the scientist may succeed where the philosopher has failed is based on the fact that the former is both rational and empirical where the latter is only rational.[1]

A psychologist, none other than William James, indicated in his book on *Pragmatism* (13) this hope of new advances, where he says, 'Empiricism gives facts without religion. Rationalism gives religion without facts.' For he recognized that rationalism alone was not enough in building any new ethical system. There must be also research about facts, indeed about the nature of human needs and social goals. In dedicating this work to J. S. Mill, William James clearly indicated that the pragmatic development lay in the main stream of effort toward a rational, though not a rationalistic ethics. I have attempted elsewhere (2) to suggest some further steps which might be feasible in the attempt to establish non-arbitrary goals as a basis for rational and empirical ethics. Actually, once a basic goal or series of goals has been found by investigation, the calculation of the degree of ethical desirability of any particular intermediate objective or course of social action depends on knowledge of the same principles as are used in technical scientific prediction, as has already been pointed out above. Consequently the social scientist, and particularly the social psychologist, is better fitted than any other specialist to inherit from the philosopher the task of developing a science of ethics.

At present a concession to practicality needs to be made to the extent that the social scientist can sometimes name only the *proximate* value goal rather than the more remote one to which it is contributory. This statement of a number of proximate goals leaves the writer open to evidence of inconsistency of values when a more advanced ethical science appears, for it may show that they conflict and fail to subsidiate to a consistent final value. Nevertheless, a statement of sincerely pursued inconsistencies is better than no statement of ethical values at all. Parenthetically, one may appropriately point out here that the constant reference to an ethical *goal* does not imply any separability of *ends and means* in the sense used by Huxley (10), since the term *ends* there refers to other than ethical ends. The *way* in which an ordinary end is achieved, e.g. whether 'prejudices' are reduced by fair or unfair means, is in general ethically more important than the end itself: but both ends and means are proximate goals on the way to some ultimate ethical value.

The idea that the integration of science and ethics may be achieved by making science the source of ethics, is, of course, a completely revolutionary one, and I cannot expect that it will be well received. The conventional view, after all, is that science is an ethically neutral and impotent study of mechanics, needing to have life breathed into it by the arts

[1] Typical of the bolder recent attempts to treat ethics scientifically is the work of Northrop (16), who expresses the conviction that 'the normative theories can be formulated as objectively and scientifically as the factual theories'. However, it should be made clear at the outset that what I mean above by treating ethics scientifically is something more than the avoidance of ethnocentrism and the objective determination of the ethical values of a culture along with its behaviour generally. This treatment of the ethics of each culture as a normative system, to be objectively investigated, is a proper attitude for the student, but it need not lead us to believe that 'all ethics are relative'. They *should* be relative only in the sense that the same harbour has different compass bearings from ships in different positions. The task of scientific ethics is to determine the true goal and the true bearings.

nd the humanities. This traditional view, to my mind, misses the essential spirit of science. t is the product of drift and superficial thinking and offers in the end nothing but the ncertain, impulsive, intuitions of the extravert in place of the longer, harder but surer oad which science has to build in its deeper, more disciplined appreciation of the meaning f truth.

The only obstacle standing in the way of a wholehearted advocacy of such a replacement f intuitive by scientific ethics is that the necessary delay in developing the latter would eave social science, in the meantime, paralysed as regards its social applicability. It is not ny intention to advocate that social science be hobbled, even temporarily, in its possiilities of improving society. Let us by all means proceed contingently with intuitive noral values, but let us state them clearly and let the advocate of particular social experiients remember that he is proceeding just as much by faith and guess work as Lenin or Iohammed, Brigham Young, or Hitler. Moreover, he is no more entitled than these nonscientific planners of new societies and new orders to call his recommended society 'a scientific experiment'—unless he arranges for careful comparative records and is willing t any moment to admit that his tentative values were wrong. And if his values happen o be democratic—in the sense that the action of the group must follow the will of the najority—he must inveigle no participator into the experiment who is not also clearly ware that the values are tentative and speculative.

Although the view of some physical scientists that social science is a degenerate ousin is justified by those social scientists who mix technical laws and ethical laws n inextricable confusion, the problem of separating technical from ethical recommendaions nevertheless exists in exactly the same form in physical science. The physical nd the social sciences differ in this respect only in degree, not in kind—namely in the egree to which ethical questions are difficult to disentangle. To release atomic energy and o decide to what use it shall be put are relatively separable issues, whereas many social cientists seem to have extraordinary difficulty in separating the scientific fact that men re not born equal from the value judgement that they must be given equal opportunity.

Ultimately, therefore, the need for a scientific ethics exists in all science. But it is the ocial sciences—especially if they make it an ideal invariably to require an exact statement f ethical 'intrusions'—that can most insistently call attention to the void where scientific thics should be, and thus promote a creation necessary to the completeness of all science.

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(*Manuscript received* 2 *February* 1950)

EMOTIONAL ATTITUDES TOWARDS THE FAMILY OF NORMAL, NEUROTIC AND DELINQUENT CHILDREN

PART I

By LYDIA JACKSON

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I. Introduction

Psycho-therapeutic work with children makes the therapist aware of the existence of certain differences in emotional attitudes not only among individual children, but also between groups of children, especially those whom we technically describe as 'neurotic' on the one hand, and as 'delinquent' on the other. Even when timidity, evasiveness, inhibition of activity and speech, or fearfulness, are among their principal symptoms, there are signs of some inner dynamics acting on these children and only imperfectly contained, perhaps by fear.

One of the effects of treatment is to release this dynamic inner pressure, which often results in a period of aggressive, destructively self-assertive behaviour on the part of the patient. As treatment continues, however, this behaviour undergoes transformation, and from socially hostile and materially destructive, gradually becomes co-operative and constructive.

What has happened? How has the force, which had manifested itself in destructive activities and emotions, become transformed into a predominantly creative force?

No simple answer, of course, can be given, but an hypothesis suggests itself that this driving force, in its primary form, was neither a-social, nor destructive, but has assumed such forms under the influence of some factors, probably acting on the patient from very early years.

The behaviour of the patient changes under treatment. These changes are usually said to be due to a resolution of the patient's conflicts and the resulting integration of conflicting urges. This, however, can only happen through the patient's relationship with the therapist. It hardly needs to be stressed that to a child patient the therapist embodies some of the parental attributes, and that one of the common effects of treatment is an improvement in the relationship between the patient and his real parents. It is therefore likely that the conflicts which manifested themselves in neurotic symptoms and in delinquent behaviour were associated with disturbances in parent-child relationship.

All this may be too self-evident to need re-statement. Yet, an objective verification of this hypothesis was desirable. The 'curve of aggressive behaviour' in the case of the delinquent children appeared to be different from that of the neurotic, the former tending to have 'plateaux' of aggression, whereas the latter frequently showed a sharply rising, and just as sharply falling, curve. The attitude of the delinquents towards the therapist

was also different from that of the neurotic patients: the former were much more detached, whereas the latter often displayed intense ambivalence. The emotional attitudes and behaviour of both groups needed comparing with a 'control' group of children who were neither neurotic not delinquent.

In search of a method which would circumvent some of the difficulties of a clinical study (its slowness, and a virtual impossibility of securing a control group of 'normal' children for regular study), the writer decided on a projection 'test', similar to the one she had employed for the study of sado-masochistic attitudes in normal and delinquent girls.[1] This test was designed to probe the subjects' emotional attitudes to their parents and siblings, and their fantasies and ideas concerning their families' attitudes towards themselves. It was hoped that the study might throw some light on the particular factors in family relationships which are associated with (*a*) self-assertion through positive achievement, (*b*) deviation of self-assertion into neurotic symptoms, (*c*) deviation of self-assertion into delinquent behaviour.

II. Description of the 'test of family attitudes'

The test consists in a specially designed set of six black-and-white line drawings, which are presented to the subjects, who are asked to make up stories on them. The pictures were designed to represent fairly common situations between adults and children and to probe the following aspects of family life: (*a*) the child's dependence on the mother (Picture 0); (*b*) the child's exclusion from intimacy between the parents with a consequent threat to its security (Picture 1); (*c*) jealousy of a sibling (Picture 2); transgression and the ensuing loneliness and guilt (Picture 3); (*e*) fear of aggression from the parents (Picture 4); (*f*) longing for the forbidden and the threat of punishment (Picture 5).

The pictures were executed by an artist[2] in accordance with the writer's instructions, in such a way as to suggest a familiar situation, yet leave room for alternative interpretations.

The age groups for which the test was thought to be especially suitable were those between 6 and 12 years. The six pictures constituting the test were identical for boys and girls, with the exception of Picture 2, where the older child was a boy in the set used for testing boys, whereas in the set used for girls, the same child was a girl. This was done to facilitate projection.

III. Experimental procedure

The whole test of six pictures was given to the subject, whenever possible, within a single interview, and took from 30 to 40 min. to administer. The instructions to the subject were given in simple language, the experimenter saying that 'It need not be a very good story, but it must be your own, and not something you've read in a book. You can say anything you like, anything that comes into your head. Remember, it must be a story *about* the picture. I don't want you to tell me what you can see in the picture because, of course, I can see it myself.'

No length limit was set on the stories thus composed, but if the story became too diffuse, the experimenter would urge the child to keep to the main subject. The length of the story varied considerably with the personality and age of the child, the range of variations in length being from about 20 words by the youngest and most inhibited, to as many as 310 words by the oldest and more articulate and confident.

[1] *Brit. J. Med. Psychol.* March 1950.

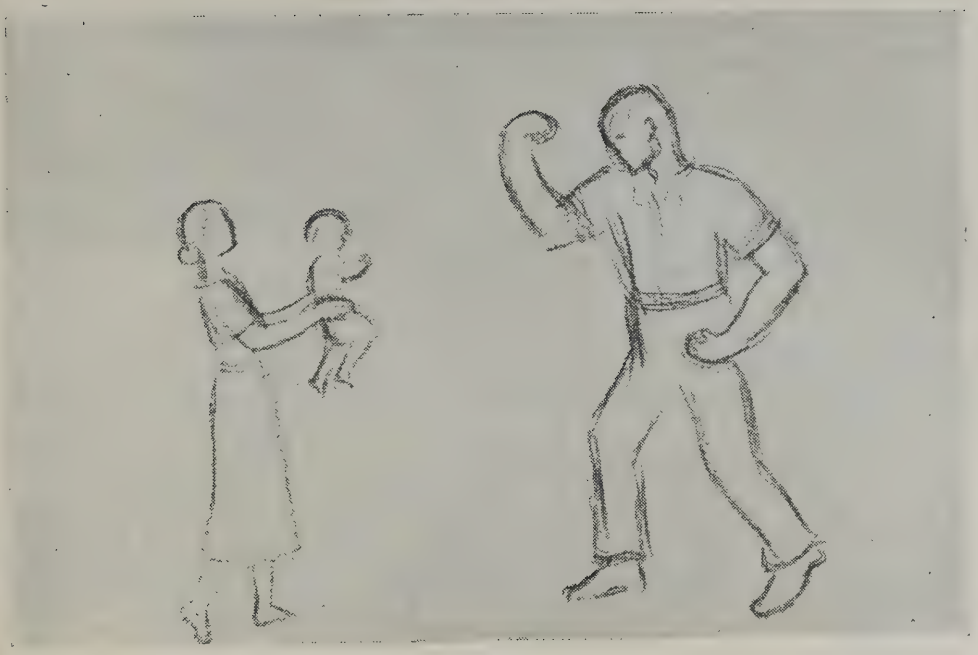
[2] Mrs Kay Gardiner.

Picture 0

Picture 1

Picture 2

Picture 3



Picture 4

Picture 5

In most cases the stories were taken down verbatim, as the child composed them, but in some cases it was found necessary to write them immediately after the child had left the room, for some young children, as well as some delinquent and neurotic subjects, were noticeably disturbed by the experimenter taking notes.

Questions were sometimes necessary to start off the more inhibited subjects. These were always the same, for instance: 'What do you think these people are doing?' or 'Who do you think this person is to the child?'. In such cases, as well as when questions were asked in the course of the story, both question and answer were recorded.

No such questions were asked if the emotional attitudes under investigation became manifest in the course of spontaneous composition.

IV. The groups of children tested

Three groups of children aged between 6 and 12 were tested. The composition of the groups and the distribution of ages and intelligence quotients in the three groups is shown in Tables 1 and 2.

Table 1. *The* I.Q. *distribution among the* 110 *subjects tested with the family attitudes test*

Subjects	Number of subjects with an I.Q. of					Total
	Under 85	85–100	101–115	116–130	Over 130	
Normal boys	—	11	3	6	—	20
Normal girls	—	11	2	5	2	20
Neurotic boys	—	4	9	4	3	20
Neurotic girls	2	8	8	2	—	20
Delinquent boys	1	11	4	4	—	20
Delinquent girls	—	4	6	—	—	10
Total number of subjects	3	49	32	21	5	110

Table 2. *The age distribution among* 110 *subjects tested with the family attitudes test*

Subjects	Number of subjects aged (yr.)					Total
	Under 6	6–8	8–10	10–12	Over 12	
Normal boys	—	6	7	7	—	20
Normal girls	—	7	7	6	—	20
Neurotic boys	—	6	9	5	—	20
Neurotic girls	1	8	8	3	—	20
Delinquent boys	—	5	5	7	3	20
Delinquent girls	—	3	3	2	2	10
Total number of subjects	1	35	39	30	5	110

With the exception of a few children of middle-class parents, the great majority in all three groups belonged to the manual worker or skilled artisan class, and were attending elementary schools. The 'delinquent' and the 'neurotic' groups were drawn from cases attending child guidance clinics, and many of them were the writer's patients. The distinction between the two latter groups was made on the basis of manifest symptoms: children who pilfered, truanted or did wilful damage to property were classified as 'delinquent', whereas children who suffered from habit disorders or other psycho-neurotic symptoms, were placed into the 'neurotic' group. The children in the delinquent group often had other symptoms besides delinquency, but the neurotic group were free from the specific symptoms characteristic of the delinquents.

Information about the family background of the subjects of the normal group was obtained through conversation with each child, following on the completion of the test, as well as from the child's teachers. In the delinquent and the neurotic groups, developmental and social histories of the great majority of the subjects were available in the form of detailed clinical records.

V. THE SUBJECTS' ATTITUDE TO THE TEST

By far the greater majority of the subjects in all three groups were interested in the test and willing to co-operate. The highest degree of co-operation was shown by the normal group. Not one of the forty children in that group refused to make up stories or showed pronounced inhibition in the attempt. The delinquents, as a group, took the test much less willingly: many were suspicious and on their guard. Several attempted to evade composition and to limit their responses to a simple description of objects and persons represented in the pictures. Some tended to answer the experimenter's questions with a brief 'yes' or 'no'. The neurotic subjects, as a group, varied most in their attitude to the test. Many showed reluctance to begin and expressed doubts concerning their ability to compose, in phrases such as these: 'I can't do it', 'It's no good my trying'. Some were inhibited and fearful, and had to be given much reassurance to induce them to carry on with the story after they had begun. On the other hand, there were many who were eager to do what they were asked and needed no urging or questions to prompt them.

VI. EVIDENCE FOR THE OCCURRENCE OF PROJECTION

There was hardly a child among the 110 tested who failed to reveal in his stories something quite personal and peculiar to himself and his home situation. This was true of the normal children as well as of the delinquent and the neurotic. The most frequent and striking examples, were, perhaps, provided by the neurotic subjects, whose emotional problems were especially pressing or who had suffered a serious trauma.

Projection took a variety of forms. Sometimes the subject's actual traumatic experience was described in a distorted form; sometimes the conflict from which the subject was suffering was ascribed to the child in the story. The subject's attempts to deal with his conflicts, such as affection for, and jealousy of, a sibling, feelings of inferiority and urge to assert himself, aggressive wishes against siblings and parents and fear of retaliation, were often projected. There were frequent open references to the subject's own behaviour and feelings, and in one or two cases, the subject spoke of the persons in the pictures as his parents and himself.

The following stories may serve as examples of different types of projection:

Projection of traumatic experience. Stories by M.S., male, aged 7 *years* 6 *months,* I.Q. 101, *suffering from compulsive movements of hands and fingers. At the age of* 5, *he fell into a fire and was severely burned about the hands.*

(*Picture* 4.) He wants to kill the woman. (Why?) Because she won't give him the baby. (Why does he want it?) *He wants to put it in the fire and cook it.* (Why does he want to do that?) He wants to have another wife and baby; he doesn't like these.

(*Picture* 5.) A man is coming to thrash the child. He's going *to put her in the fire and burn her.* (Who's he?) He's her Dad. (Why does he want to do that to her?) Because he doesn't like her.

Projection of Oedipus conflict. A story by E.C., male, aged 6 *years* 10 *months,* I.Q. 131, *suffering from night fears and other symptoms.*

(*Picture* 3.) This story starts not about the picture. There was once a little boy. His Mummy told him: 'Go down and get me some coal.' He said: 'No, I won't.' She said: 'I do things for you, why shouldn't you do things for me?' 'No, you don't.' His Daddy then came in, and she told him, and he gave the little boy a good hiding (with much emphasis and gusto), and took him upstairs and put him in the nursery. He's sitting alone on a stool. ('Is the door locked or unlocked?') Yes, the door is locked. ('What is he thinking?') He's thinking: 'I wish there were some real magic spells, so that I could become a big prince and have a sword and kill her.' ('Kill whom—his Mummy?') This Mummy. And Daddy heard him. He was listening through the keyhole at the door. The boy was saying it quietly to himself, and his Daddy heard him. He came in and gave him a harder smack. ('Where is his Mummy?') His Mummy is in the kitchen. The little boy then made up his mind to run away. (And did he?) Yes, he ran away and bought himself a gun at a fair. He made himself a sword. He became a wicked man. He went back and told his Mummy: 'See, now I can kill you.' Then Daddy came in. The boy stood there, and he took his sword and killed his Daddy.

Projection of conflict between affection for, and jealousy of, a sibling. A story by M.S., female, aged 7 *years* 2 *months*, I.Q. 125, *normal.*

(*Picture* 2.) There was once a little boy and a little girl, and mother and father. And *the baby loved* father and mother and the little girl very much, as well. The little girl was very good always. (What is she thinking?) She's thinking *how she would love to be a little baby again, because she likes the little baby so much.* (Who do father and mother like best?) Father and mother like them both best. The little boy is sometimes naughty.

Projection of feeling of being rejected on account of wrong sex. A story by P.S., male, aged 10 *years* 7 *months*, I.Q. 118, *a pilferer, suffering from severe outbursts of temper, usually against his mother and younger sister.*

(*Picture* 4.) There was once a woman who had a baby and she liked it very much. But the man didn't like it, and he puts his fists to its face. He doesn't like it because *he wanted a girl, and the baby is a boy.* So he tells the woman to get rid of him'.

Projection of inferiority feelings. A story by M.J., male, aged 10 *years* 9 *months*, I.Q. 103, *unstable, suffering from fears, very backward at school.*

(*Picture* 1.) Mother and father are discussing things about the children, about the elder boy. *He's got to be a road-sweeper—it's the only thing he's fit for.* He is listening behind the door. He suddenly makes an attack and knocks father and mother out, and the little brother, too. Then he runs away and gets married and becomes an architect.

The information which the writer had about the real happenings in the families of these children confirmed that the stories were reflections of genuine emotional experiences.

VII. The chief psychological mechanisms involved

It thus appears that the chief psychological mechanism involved in the subjects' responses to this test was 'projection', but there is some evidence that the mechanism of 'identification' was also at work. The term 'projection' is employed by the writer in its meaning of ascribing one's own feelings, thoughts or wishes to another person, in this case to persons in the pictures. The term 'identification' is employed in its meaning of putting oneself into another person's place and behaving as if one were that other person.

In classifying the subjects' responses it could not be assumed that the working of these mechanisms would be uniform in all cases, or even in the same individual case. It seemed most probable that the subject would ascribe his mental attitudes to the person in the picture who most resembled himself. For that reason, a child figure was included in every picture of the test, with the exception of Picture 0, where a cradle, presumably containing a baby, takes the child's place. On these grounds, the writer decided that some of the responses needed to be classified in accordance with two, or even three, possible meanings.

For example, in a story on Picture 4, the response 'This man is going to kill this little girl. They're going to kill her' (G.P., male, aged 9 years 5 months, I.Q. 115), could be interpreted as (*a*) a fantasy of parental hostility against the subject, or (*b*) as the subject's hostility against the younger sibling. In a story on Picture 3, the response 'This little boy has been naughty. He threw the baby out of the window. The little boy will go to prison. His father and mother will say "Good!"' (M. J., female, aged 7 years 1 month, I.Q. 101) could be interpreted as (*a*) an expression of the subject's hostility towards the younger sibling, (*b*) the subject's idea of punishment she deserves for such hostility, or (*c*) the subject's feeling of being rejected by her parents. In the examples quoted, the subjects' personal histories, the character of their symptoms and their families' set-up, warranted the reading of these several meanings into their stories.

Similar reasoning applies to the working of the psychological mechanism of identification. It is presumably easy to put oneself in the place of someone with whom one has affinity, but probably also of someone whose place one wishes to occupy, and whose accomplishments and privileges one envies. The choice of the object of identification, as we know from psycho-analytical literature, is usually determined by a set of circumstances peculiar to each individual case. In his responses to Picture 4, for instance, the subject might identify himself with the child maltreated by the parents, or with the adult figure punishing the 'naughty' child, perhaps a younger sibling, for some misdemeanour.

Nor can we be certain that such identification will be sustained throughout the story on the same picture. It may be switched over from one figure to another. Thus, a delinquent boy, R.P., aged 6 years 10 months, with an I.Q. of 90, whose symptoms included persistent pilfering (despite 'thrashings'), setting fire to things in the home, and aggressive jealousy of a younger sister, produced the following story:

(*Picture* 5.) This little boy is playing with the fire. Like my sister does. This big boy is going to hit her, because he doesn't like her. The little girl is a naughty one. She has a piece of paper in her hand: you can't see it. She's going to light it and make a fire in the street. She wants to burn people. (Why?) She wants to burn people so that she can get their money out of their pockets. (What does she want money for?) She wants money to buy airplanes.

It appears that at the beginning of his story the subject identifies himself with the child in the picture, 'the little boy' who 'is playing with the fire'. Then 'the little boy' becomes a girl, presumably his sister, on whom he projects his 'naughty' wish to 'light a fire in the street', and identification is switched over to the male figure. Other 'bad' wishes are also projected on 'the little girl': 'to get money out of people's pockets' and 'to buy airplanes'. For such wishes she deserves punishment, and *he* is going to punish her. The child in the picture is, thus, both himself and his sister, whom he would like to see punished for 'naughty' wishes of which he himself is guilty.

VIII. Classification and computation of data

The data obtained from 110 subjects consisted of 652 'stories' which varied from 20 to 300 words in length. The classification of the responses had to be based on the actual data, as no standard system of classification and scoring was then available for this type of projection test.

The first step in the analysis of the data was the breaking up of the stories into types of responses. The following types of responses were found:

In stories on Picture 0, nine types:

(1) Woman is baby's mother
(2) Woman is a stranger to the baby
(3) Baby is 'good' or 'nice'
(4) Baby is 'bad' or 'naughty'
(5) Woman is kind to baby
(6) Woman is unkind to baby
(7) Father of the baby is at home or at work
(8) Father is away or dead
(9) Mother is away or dead.

In stories on Picture 1, sixteen types:

(1) Child 'naughty'
(2) Child 'good' or 'nice'
(3) Child sometimes good, sometimes naughty
(4) Transgression serious
(5) Transgression minor
(6) Punishment severe
(7) Punishment mild
(8) No punishment
(9) Man outside dangerous
(10) Man outside relative or friend
(11) Parents ambivalent towards child
(12) Child ambivalent towards parents
(13) Parents reject child
(14) Parents are fond of child
(15) Child hostile to parents
(16) Child fond of parents.

In stories on Picture 2, twelve types:

(1) Older child rejects baby
(2) Older child likes baby
(3) Older child jealous of baby
(4) Older child accepts baby
(5) Older child is 'good' or 'nice'
(6) Older child is 'naughty' or 'bad'
(7) Older child is sometimes good, sometimes naughty
(8) Parents prefer baby
(9) Parents prefer older child
(10) Parents like both equally
(11) Older child rejects parents
(12) Parents reject older child.

In stories on Picture 3, twelve types:

(1) Transgression serious
(2) Transgression minor
(3) No transgression. Alone by choice
(4) Loneliness or bereavement
(5) Remorse or regret shown
(6) No remorse or regret shown
(7) Punishment severe
(8) Punishment light
(9) Parents away or dead
(10) Parents near
(11) Door locked
(12) Door unlocked or not mentioned.

In stories on Picture 4, nine types:

(1) Situation of danger
(2) Situation of play or co-operation
(3) Child 'naughty' or 'bad'
(4) Child 'good' or 'nice'
(5) Child rejected by *one* parent
(6) Child rejected by *both* parents or parent-figures
(7) Child accepted by both parents
(8) Child maltreated
(9) Child treated well.

In stories on Picture 5, ten types:

(1) Child 'naughty' or aggressive
(2) Child playing or industrious (helping parents)
(3) Parent punishing or forbidding
(4) Parent playful or protective
(5) Male figure—hostile sibling or stranger
(6) Male figure—friendly sibling or stranger
(7) Mother against child
(8) Mother on child's side
(9) Mother at home or near at hand
(10) Mother away or dead.

The second step in analysis consisted in computing the responses of each type for each of the three groups of subjects. A separate tabulation of the data for boys and girls failed to reveal significant differences between the sexes, and the two sets of figures were then added together and dealt with as sums of responses for the whole group.

The third step consisted in comparing the numbers of responses of each type given by the subjects belonging to each of the three groups. Some of the totals were not appreciably different, but many suggested a probability of a significant difference, and these were statistically analysed.

The statistical method employed was Fisher's χ^2 test. χ^2 was calculated for each response

in turn, assuming that the number of responses given by the normal subjects represents the 'expected' possibility, and that the numbers given by the neurotic and the delinquent subjects are the 'observed' values.

A value of $P < 0{\cdot}01$ was taken as a criterion of significance.[1] Table 3 shows in columns (iv) and (v) whether the differences are significant or not.

Table 3. *Data for forty normal, forty neurotic and thirty delinquent subjects.*

(Columns (i), (ii) and (iii) give the number of subjects whose responses were those on the left of the table: columns (iv) and (v) indicate whether the differences between (i) and (ii) or between (i) and (iii) are significant (χ^2 test). A value $P < 0{\cdot}01$ is considered highly significant: other values are not significant.)

No. of picture	No. of item	Responses	Number of subjects giving the response: Normal (i)	Neurotic (ii)	Delinquent (iii)	Significance: (iv)	(v)
0	1	Woman—baby's mother	38	28	24	—	—
0	2	Woman—a stranger	1	1	3	—	—
0	3	Baby 'good' or 'nice'	36	21	20	$P<0{\cdot}01$	—
0	4	Woman kind to baby	37	19	22	$P<0{\cdot}01$	—
1	5	Child 'naughty' or 'bad'	8	14	12	—	$P<0{\cdot}01$
1	6	Child 'good' or 'nice'	18	7	4	$P<0{\cdot}01$	$P<0{\cdot}01$
1	7	Punishment severe	2	8	9	$P<0{\cdot}01$	$P<0{\cdot}01$
1	8	No punishment	8	3	1	—	$P<0{\cdot}01$
1	9	Man outside dangerous	2	21	14	$P<0{\cdot}01$	$P<0{\cdot}01$
1	10	Man outside relative or friend	38	23	19	—	—
1	11	Parents ambivalent	17	10	12	—	—
1	12	Parents reject child	1	10	8	$P<0{\cdot}01$	$P<0{\cdot}01$
1	13	Parents are fond of child	32	13	9	$P<0{\cdot}01$	$P<0{\cdot}01$
1	14	Child hostile to parents	2	13	3	$P<0{\cdot}01$	—
1	15	Child fond of parents	25	9	8	$P<0{\cdot}01$	$P<0{\cdot}01$
2	16	Older child jealous of baby	11	19	10	—	—
2	17	Older child likes the baby	30	13	14	$P<0{\cdot}01$	—
2	18	Older child rejects the baby	6	14	10	$P<0{\cdot}01$	$P<0{\cdot}01$
2	19	Older child is 'good'	18	10	7	—	—
2	20	Older child is 'naughty'	3	13	10	$P<0{\cdot}01$	$P<0{\cdot}01$
2	21	Parents like both children equally	14	9	7	—	—
2	22	Older child rejects parents	4	13	7	$P<0{\cdot}01$	—
2	23	Parents reject older child	5	11	12	$P<0{\cdot}01$	$P<0{\cdot}01$
3	24	Remorse or regret shown	16	9	9	—	—
3	25	No remorse or regret shown	7	22	17	$P<0{\cdot}01$	$P<0{\cdot}01$
3	26	Punishment severe	6	16	16	$P<0{\cdot}01$	$P<0{\cdot}01$
3	27	Punishment light	25	13	9	—	$P<0{\cdot}01$
3	28	No trangression. Alone by choice	12	7	3	—	—
3	29	Parents far away or dead	5	17	10	$P<0{\cdot}01$	—
3	30	Parents near	36	23	20	—	—
3	31	Door unlocked or not mentioned	35	26	14	—	$P<0{\cdot}01$
3	32	Door locked	7	14	16	—	$P<0{\cdot}01$
4	33	Situation of danger	12	31	19	$P<0{\cdot}01$	$P<0{\cdot}01$
4	34	Situation of play or co-operation	17	9	11	—	—
4	35	Child 'good' or 'nice'	32	12	5	$P<0{\cdot}01$	$P<0{\cdot}01$
4	36	Child rejected by both parents or parent figures	1	13	5	$P<0{\cdot}01$	$P<0{\cdot}01$
4	37	Child accepted by both parents	31	10	12	$P<0{\cdot}01$	$P<0{\cdot}01$
4	38	Child maltreated	6	24	13	$P<0{\cdot}01$	—
4	39	Child treated well	34	8	15	$P<0{\cdot}01$	—
5	40	Child playing or industrious	24	14	6	—	$P<0{\cdot}01$
5	41	Parent playful or protective	21	11	3	—	$P<0{\cdot}01$
5	42	Mother on child's side	11	4	7	—	—
5	43	Man in the picture hostile sibling or stranger	7	14	9	$P<0{\cdot}01$	—

IX. Examination of the data contained in Table 3

The writer proposes to examine the data of Table 3 in the following order:

(*a*) Types of response which showed significant differences for *both* the neurotic and the delinquent subjects relative to the normal group.

(*b*) Those that showed significant differences *only* between the normal and the neurotic subjects.

(*c*) Those that showed significant differences *only* between the normal and the delinquent subjects.

(*d*) Those that showed such differences between the neurotic and the delinquent subjects, in so far as these differences have not been brought out by the examination of (*b*) and (*c*).

(*e*) Those that showed no significant differences between the three groups.

A summary of conclusions will be given at the end of this paper.

(*a*) *The types of responses which showed significant differences between the normal group on the one hand, and the neurotic and delinquent groups, on the other*

	Type of response	Given by the neurotic and delinquent
Picture 1	1. Child 'good' or 'nice'	*Less* frequently than by the normal
	2. Punishment severe	*More* „ „ „
	3. Man outside dangerous	*More* „ „ „
	4. Parents reject child	*More* „ „ „
	5. Parents fond of child	*Less* „ „ „
	6. Child fond of parents	*Less* „ „ „
Picture 2	7. Older child rejects baby	*More* „ „ „
	8. Parents reject older child	*More* „ „ „
	9. Older child is naughty	*More* „ „ „
Picture 3	10. No regret or remorse shown	*More* „ „ „
	11. Punishment severe	*More* „ „ „
Picture 4	12. Situation of danger	*More* „ „ „
	13. Child 'good' or 'nice'	*Less* „ „ „
	14. Child rejected by both parents or parent-figures	*More* „ „ „
	15. Child accepted by both parents	*Less* „ „ „

An examination of these data shows the presence of certain trends in both the neurotic and the delinquent subjects which distinguish them from the normal. If the subjects identify themselves with the child in these pictures (and of this there is much evidence in their stories), then items 1 and 13 show that the normal children feel 'good' or 'nice' much more frequently than the delinquent or the neurotic. Even if they do not necessarily identify themselves with the child in the pictures, but view him as some child they know, or as a sibling, they think him to be 'good' or 'nice', rather than the reverse.

When they see the adult figures in Pictures 1 and 4 as the parents of the child, they describe these parents as being fond of the child, and the child as fond of the parents. They see the child as accepted by both parents. This is evident from items 5, 6 and 15.

The neurotic and delinquent subjects, on the other hand, tend to feel rejected, or to wish that the child in the picture, who may stand for a sibling, should be rejected by the parents. This is evident from the items 4, 8 and 14. When they compose stories about 'a naughty child', they prescribe severe punishment for his transgression. This is shown in items 2 and 11. There is usually a lack of proportion between the relative triviality of transgression and the severity of punishment. Thus, a boy 'is going to be thrashed because he teased his little sister' (S.G., male, aged 8 years 11 months, I.Q. 113), or a girl 'is going to be put in a Home' because she innocently told her father something her mother wanted her to conceal (E.D., female, aged 11 years 1 month, I.Q. 102).

There is obviously a connexion between their feelings of rejection and their feeling of 'badness', for if they identify themselves with the child in the pictures, they tend to see that child as 'naughty'. If, on the other hand, they see that child as a sibling or play companion, they project their naughtiness on to him, as item 9 shows. At the same time, when that child is seen as having committed some transgression, he is more often than not

viewed as unrepentant, as is shown by item 10. The thoughts ascribed to the child are often rebellious thoughts, such as 'He's thinking how to get out' (B.D., male, aged 6 years 11 months, I.Q. 100); or 'He's thinking he's got a bad mother' (T.F., male, aged 9 years 8 months, I.Q. 130). This 'naughty' child does not want a younger brother or sister, as is clear from item 7. As it might be expected, probably because of his hostility, his 'naughtiness' and his awareness of parental rejection, he is very insecure and views the ambiguous male figure in Pictures 1 and 4 as dangerous (items 3 and 12). On these figures become centred his projected aggression, his expectation of punishment arising from his feeling of guilt, and his fear of the avenging parent.

These trends and tendencies, as manifested in the test, distinguish both the delinquent and the neurotic child subjects from the normal children.

(*b*) *The types of responses which showed significant differences between the normal and the neurotic groups* (*but not between the normal and the delinquent*)

	Type of response	Given by the neurotic subjects
Picture 0	16. Woman kind to baby	*Less* frequently than by the normal
Picture 1	17. Child hostile to parents	*More* ,, ,, ,,
Picture 2	18. Older child likes baby	*Less* ,, ,, ,,
	19. Older child rejects parents	*More* ,, ,, ,,
Picture 3	20. Parents far away or dead	*More* ,, ,, ,,
Picture 4	21. Child maltreated	*More* ,, ,, ,,
	22. Child treated well	*Less* ,, ,, ,,
Picture 5	23. Man in the picture hostile sibling or stranger	*More* ,, ,, ,,

The examination of these data shows that, in addition to feeling rejected by the parents, the neurotic children in this group also tend to feel maltreated by them, in so far as they identify themselves with the child in the pictures, whereas the normal feel they are treated well. This is shown by items 21 and 22, respectively. If the child stands for a sibling, their choice of this response is in line with the earlier evidence of sibling hostility. This is confirmed by item 18. Item 23 provided additional evidence of the neurotic children's insecurity, their expectation of aggression from others, be it strangers or siblings. Even the mother's relationship with her baby is not seen as very secure, as is shown by item 16. If the baby stands for a younger sibling, the neurotic children apparently wish the mother not to be kind to it. Items 17 and 19 illustrate the projection by these children of their own hostility towards their parents on to the child in the picture. That they unconsciously expect to be punished for their hostility by parental desertion, or by realization of their 'death wishes', can be seen from item 20.

We may ask at this stage why the delinquents, who shared several other trends with the neurotic subjects, do not significantly differ from the normal in the trends just examined. They do not show as much hostility towards parents and siblings, as much anxiety or as much self pity (item 21), as the neurotics. This need not necessarily mean that the delinquents are, in certain respects, more like the normal than the neurotic children. It may mean, however, that the direction of their emotional drives is centrifugal rather than centripetal, away from the family, instead of towards the family, as is the case with the neurotics. They thus appear more detached from the family. On the other hand, it is possible that this detachment is a form of defence and that hostile emotions are deeply repressed, which gives the delinquents' responses a superficial similarity to the responses of the normal. The relative infrequency of hostile, anxious and self-pitying responses in the stories of the normal is less likely to be due to repression alone: items 16, 18 and 22,

as well as several items in section (*a*), indicate better integration and overcoming of ambivalence by the normal.

In some responses, however, the delinquents and *not* the neurotics differed significantly from the normal, and these responses will be examined next.

(*c*) *The types of responses which showed significant differences between the normal and the delinquent groups* (*but not between the normal and the neurotic*)

	Type of response	Given by the delinquent subjects
Picture 1	24. Child 'naughty' or 'bad'	*More* frequently than by the normal
	25. No punishment	*Less* ,, ,, ,,
Picture 3	26. Punishment light	*Less* ,, ,, ,,
	27. Door unlocked or not mentioned	*Less* ,, ,,
	28. Door locked	*More* ,, ,, ,,
Picture 5	29. Child playing or industrious	*Less* ,, ,, ,,
	30. Parents playful or protective	*Less* ,, ,, ,,

The examination of these data shows that the delinquents exhibit a pronounced trend towards 'bad identification', or, if the child in the picture stands for a sibling or play companion, they tend to project their own feeling of 'badness' on to him. This child is apparently regarded as dangerous, hence the fantasy of his being locked in (items 27 and 28). They find it difficult to imagine such a child to be innocently playing or helping someone (item 29), or to think that the parent might be playing with, or protecting the naughty child (item 30). They cannot imagine this child being forgiven when it had been guilty of some transgression (item 25), or of receiving light punishment in such a case (item 26).

These data reflect, most likely, actual situations in the delinquents' families. The delinquent children are more liable to be scolded, punished and told they are 'bad' or 'naughty' than the normal, or even the neurotic children. They are less likely than the latter to be forgiven or punished lightly for their transgressions because their behaviour arouses more alarm and hostility in the adult than the less anti-social transgressions of non-delinquent children. Their parents are not likely to be as companionable as those of the normal or even the neurotic children. Delinquents are, without doubt, sometimes threatened with 'locking up', even if the threat is rarely carried out; and because their freedom is curtailed more often than that of other children, they would tend to feel 'bad' and 'dangerous'.

(*d*) *The types of responses which showed differences between the delinquent and the neurotic groups of subjects*

All the above may appear self-evident in so far as the comparison is limited to the normal and the delinquent subjects, but when we examine the differences between the responses of the delinquent on the one hand, and the neurotic on the other, we see that somewhat subtler, less easily definable differences are revealed. Thus, the delinquents draw a picture of a harsher family background than do the neurotics. At the same time, they appear to accept this situation with greater equanimity, or at least with less overt hostility, as if their sensibilities were less developed, or already blunted. The neurotics, on the other hand, give evidence of more intense emotional reactions to the parents and siblings than the delinquents, which suggests that the emotional ties binding them to their families are much closer.

It is true that both groups tend to feel rejected by their parents, but while the neurotics react to this by anxiety (i.e. fantasies of death of the parents, or separation from them),

the delinquents seem to expect direct, concrete aggression in the form of punishment and isolation, as is shown by the last set of responses examined.

It is necessary, however, to point out that the delinquent subjects in this study were almost all clinical treatment cases, and that a great majority of them were suffering from psycho-neurotic symptoms, as well as being delinquent. Therefore, it was to be expected that their responses would, in many respects, resemble those of the neurotic subjects.

(*e*) *The types of responses which showed no significant differences between the groups*

Such responses belonged to one of the four following groups:

(i) Stock responses, such as in stories on Picture 0: 'Baby's father is out at work'.

(ii) Unusual responses, so infrequent that they could not be grouped and compared quantitatively, such as in stories on Picture 2: 'Parents prefer older child (to the baby)'.

(iii) Similar responses due to different motives, such as occurred in stories on Picture 3, when the normal, the delinquent and the neurotic gave responses, describing a 'minor transgression' on the part of the child, in an almost equal proportion. This probably was a projection of real experience on the part of the normal, whereas on the part of the delinquent the response was most likely motivated by a reluctance to mention their own, more serious transgressions; and on the part of the neurotic—by the fear of their own aggression, leading them to minimize the child's guilt.

(iv) Responses motivated by the suggestive influence of the picture, as in stories on Picture 5, where the majority of responses for all groups described the child as 'naughty' or 'aggressive'—'doing something she was told not to'.

One might add that the absence of differences between the groups in some types of responses was to be expected in a test of free composition and with groups of subjects whose social background and ordinary everyday experiences were closely similar.

X. Some qualitative differences in the responses of the three groups

Space does not allow the writer to give examples of complete sets of stories and to discuss their qualitative differences in this paper. Some such differences were, by necessity, obscured through classification of responses into 'types', which inevitably led to inclusion under the same heading of responses expressing greatly varied intensities of emotion. For example, the item 'Hostility to sibling' includes responses such as 'No, she doesn't like the baby very much' (M.C., female, aged 7 years 4 months, I.Q. 102) and 'He is thinking that he hates the baby' (K.G., male, aged 8 years 3 months, I.Q. 98).

Some of the qualitative differences are, however, evident from the frequency with which subjects belonging to different groups chose certain themes. There were five important themes, the numbers for which were computed and compared for the three groups of subjects. These themes were (i) the murder theme, (ii) the violence theme, (iii) the cruelty to baby theme, (iv) the stealing theme, and (v) the 'being sent away from home' theme.

The results obtained for the three groups were as shown in Table 4:

Table 4

Subjects	Themes (i)	(ii)	(iii)	(iv)	(v)	Responses (total)
Normal (40)	—	5	2	5	—	12
Neurotic (40)	30	18	4	5	4	61
Delinquent (30)	11	10	8	6	8	43

(The figures in columns (i), (ii), (iii), (iv) and (v) show the number of times the theme designated by this number occurred in the stories by the subjects of each group.)

The normal subjects, as might have been expected from the data examined previously, use these unpleasant themes in their stories with remarkable scarcity. In fact, two of the themes, 'murder' and 'being sent away from home' are not used by them at all. This is clearly significant and indicates their solid sense of security, as well as a strong 'reality sense' which puts a brake on fantasies of violence and rejection. The theme of 'violence' occurs only five times, compared with the neurotic subjects' eighteen, and the delinquents' ten. The 'cruelty to baby' theme occurs twice in the stories of the normal, whereas the 'stealing' theme occurs five times, that is, as frequently as it does in the neurotic group and only once less than in the delinquent group. The latter result might be due to the normal subjects' relative freedom from inhibition, as far as the subject of stealing is concerned, whereas the low figure for the delinquents might be due to their reluctance to introduce a subject which was embarrassing to them.

Otherwise these results emphasize the differences previously found between the responses of the neurotic and the delinquent groups. The figures for 'murder' and 'violence' themes are higher for the neurotics than they are for the delinquents. In these stories murder and violence are usually inflicted on children, and so are motivated, at least partially, by the anxiety characteristic of the neurotic, and by his tendency to turn his aggression on himself, in fantasy, as well as in some types of symptoms.

The delinquents, on the other hand, are the only group to show a relatively high occurrence of 'being sent away from home' theme, which may be an evidence of an actual threat, but may also indicate the feeling of being 'cut off' from the family. The figure for 'cruelty to baby' theme is also high for the delinquents.

XI. Summary and conclusions

When all these differences are reviewed in relation to the family situations pictured in the test, the following conclusions, we think, can be drawn:

The normal subjects, as a group, function more on the plane of reality than either the neurotic or the delinquent. They give the 'expected' response much more frequently. They tend to think of others, both adults and children, as 'good', rather than 'bad', but are realistic enough to admit that children are sometimes 'naughty' and are usually punished for their transgressions. This punishment, however, is rarely imagined by them as cruel or excessive. Although they tend to think of parents as ambivalent towards their children, they believe that most of the time their parents are fond of them and ready to give them help and protection. They freely admit sibling jealousy, but this does not lead to rejection of the sibling, and is overlaid by affection for it. They are sufficiently secure within their family to assume that outsiders are likely to be benevolent, and they do not suffer so obviously from fear and mistrust of strangers.

The neurotic subjects, as a group, are much less realistic in their approach to family life situations than the normal. They believe that 'naughtiness' or 'badness' are much more commonly met with in children than 'goodness' and that it is likely to be punished with a severity disproportionate to its seriousness. Their jealousy of siblings is hardly moderated by a natural liking and leads to rejecting them, and to a fear of aggression from the older more powerful sibling. They tend to believe that parents reject and maltreat their children more often than they like and protect them, and that children react to such treatment with hostility and rejection of the parents. This belief appears to increase their feelings of

insecurity, so that they are inclined to view every stranger as dangerous, and are apt to react with anxiety to any ambiguous situation.

The delinquent subjects, as a group, share with the neurotic their unrealistic approach to life situations, but in some respects show themselves to be more 'conventional', as well as more detached. Even more strongly than the neurotic subjects they tend to believe that children are 'bad' and parents ever ready to deal out severe punishment. Similarly, they are aware of parental rejection, but are less ready to recognize or to admit that children, too, feel hostile towards, and reject parents. Nor do they, as a group, show such recognition of open hostility towards, and rejection of, a rival sibling. On the other hand, fantasies of being 'shut out', isolated, or sent away, are more common among them than among the neurotics. The delinquents are also somewhat more realistic in their attitude towards 'strangers' or outsiders: they view them as dangerous somewhat less frequently than the neurotics.

In general, emotional interplay between neurotic subjects and their families, as reflected in their responses to the test, is more lively and intense than between delinquents and their families, although these emotions are of a predominantly negative character. Delinquents appear to be more detached, less deeply involved in the family loves and hatreds than are the neurotics. This may be due to the actual feeling of 'not belonging', as well as to a more thorough repression of both hostile and positive emotions.

The writer hopes to have shown that the emotional attitudes of children towards their families can be studied objectively, by means of a specially designed projection test, and that significant differences between normal, neurotic and delinquent emerge, which may have important bearings on the prophylactic and therapeutic practice. She hopes to discuss these aspects of her study in another paper, where complete examples of stories from each group of subjects will be given, with the analysis of their content and its implications for therapy and prophylaxis.

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(*Manuscript received* 20 *March* 1950)

AN ITEM-ANALYSIS OF THE ISHIHARA TEST

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I. Introduction

In order to complete a study of the Ishihara Test for colour blindness already published in part (4,10), it is necessary to add a detailed item-analysis dealing with the plates individually, and/or grouping together for study those which are similar in construction. In the previous study it was shown: (*a*) that the test as a whole was completely useless for distinguishing different degrees of colour sensitivity among normal and minor defective subjects; (*b*) that it was effective in distinguishing major defectives from normal and minor defectives; (*c*) that it was almost useless for distinguishing different types or degrees of defect among major defectives; but (*d*) that it was to some extent capable of distinguishing those major defectives with darkened red from those in whom the red is normal in brightness, though very inefficiently.

The present item-analysis will seek to answer the two further questions: (*a*) whether in general any plates of the test are more reliable than any others; and (*b*) whether any particular plates are valid for discriminating certain types of defectives. The answers to these questions will make it possible to say whether a shortened form of the test might be made with advantage, and whether such a form might be employed to indicate certain kinds of defectives with any degree of confidence.

II. Test plates, subjects and method

Test plates. The version of the Ishihara Test (Japanese printing) used in this research consists of six groups of four plates, together with two 'jokers' which are used to show the subjects what they are expected to do and to guard against malingerers who wish for some reason to pretend to be colour blind, and also six plates of the 'wavy-line' type used for subjects who cannot read (2). Plate 1 is the first 'joker', and reads '12' for all subjects, being orange on a blue background, and clear through brightness contrast even when the hues are indistinct. Only one subject in all those mentioned below found difficulty with this plate: he was a deuteranope with a superimposed total colour blindness of hysterical origin; another who had a form of colour blindness in the left eye following an accident also failed to read it with that eye (3, 5, 6, 7). The second 'joker' is Plate 32, which is a 'wavy line' figure in the same orange and blue colours as Plate 1, and it is intended for subjects who cannot read. The 'jokers' will not be discussed further.

The remaining six 'wavy-line' plates will not be discussed again. The writer has spent much time trying to use them in such a way as to detect various types and degrees of defect, but entirely without success. They are, however, reasonably good for discriminating

all types of red-green major defectives from normal and minor defective subjects, though without differentiating sub-types or degrees of defect. They are useful for subjects who are unable to read.

The plates numbered 2–25 will be dealt with in this paper, and they may be classified into six groups. Plates 2–5 show pink figures on green backgrounds, each with an alternative to be read by major defectives. Plates 6–9 show green figures on pink backgrounds, each with an alternative as before. Plates 10–13 show pink figures on green backgrounds, and Plates 14–17 show green figures on pink backgrounds, and none of these has an alternative to be read by defectives. Plates 18–21 are faintly greyish figures on multi-coloured backgrounds, to be read by major defectives but not by normal subjects. Plates 22–25 should each show two figures to the normal, and but one to the colour blind. The left figure is red and the right magenta, in each plate, and the backgrounds are dark grey. Those subjects with darkened red colour vision should read the magenta but not the red figures, while those major defectives with red of normal brightness should read the red and not the magenta figure. This is because red should be indistinguishable from dark grey or black to protanopes (or 'red blind' as Ishihara calls them), while magenta is the complementary of the green which should be indistinguishable from grey to deuteranopes (or 'green blind' as they are called by Ishihara). As shown in previous papers (4, 10), the discriminating power of these plates is very poor, because they do not allow for the varying degree of darkening of the red and the varying weakness of the green (and therefore of the magenta) in question for these types.

Subjects. These were grouped on the basis of the writer's studies of the Rayleigh Equation (8, 9) into fifteen classes. The numbers in these classes are the numbers who did the Ishihara and one of the other tests, and must not be taken as if they represented the usual frequencies in these classes. In comparison with the previous papers on the Ishihara Test (4, 10), the numbers are changed because some new subjects have been tested and added to the lists, and because those papers took no account of yellow and blue minor defects. In order to deal with yellow and blue a certain proportion of subjects who had been classed as normal or as having minor red-green defects are now better classed as minor yellow and blue defectives.

Since this classification has been explained and justified elsewhere (8, 9), it need not be discussed again here. The reason why the numbers of deviant subjects were rather small was that the writer did not realize that an item-analysis of the Ishihara Test would be of interest until other people asked him to make one, because it was already evident to him that this test would be of little value in separating the various types of defectives. Indeed this was the main stimulus which made him construct a better test. Consequently many subjects were allowed to depart without doing the Ishihara Test at all.

Method. The Ishihara Test was kept scrupulously clean, and no subjects were allowed to indicate with their finger, pen or pencil what they thought they could see on the plates. This is most important. A clean pointer which cannot mark or soil the plates is absolutely necessary for this purpose. The lighting and other general conditions have been explained before (10).

In order to interpret the results it is necessary to take into account that there is a right and wrong reading for normal subjects for each plate, and a different right and wrong reading for the major defectives (and for either group one of the right or wrong readings may be 'nothing'). In addition to this, many normal and defective subjects offer 'other'

readings, and these are in some plates fairly numerous; for example, Plate 12, which is supposed to be read as '97' by the normal and as 'nothing' by major defectives, produced all the following readings from the green anomalous subjects: '97', 'Nothing', '27', '84', '23', '25', '45', '78', '28', '36', '87', '95' and '81'. Several other plates are almost equally ambiguous, and some of those which are less ambiguous for defectives are more ambiguous for normal subjects.

Another point to be considered is that the plates are grouped into six sets, as explained above, and these should be taken separately. It seemed therefore that the most satisfactory way of dealing with the twenty-four plates was to tabulate for each plate all the readings made by subjects in each of the fifteen groups given above. In these tables all the 'normal' readings are correct for the first seven classes, whereas all the (appropriate) 'colour-blind' readings are correct for the remaining eight. For the first seven classes it is correct to make

Table 1. *Subjects Tested*

		Men	Women
I.	Absolutely normal	17	48
II.	Red deviant	19	10
III.	Green deviant	7	14
IV.	Red-green weak	15	36
V.	Yellow deviant	5	4
VI.	Blue deviant	7	2
VII.	Yellow-blue weak	33	22
	Total of normal subjects	103	136
VIII.	Green anomalous	36	6
IX.	Deviant deuteranopes	14	6
X.	Moderate deuteranopes	20	1
XI.	Extreme deuteranopes	14	0
	Total of green anomalous and deuteranopes	84	13
XII.	Red anomalous	6	1
XIII.	Red anomalous with darkened red	6	1
XIV.	Moderate protanopes	7	3
XV.	Extreme protanopes	24	0
	Total of red anomalous and protanopes	43	5
	Total of major defective subjects	127	18

no reading for Plates 18–21, and to see both figures in each of Plates 22–25. For the remaining eight classes it is correct to see the proper alternative figure for each of Plates 2–9, to make no reading for Plates 10–17, to read the proper figures in Plates 18–21 and the appropriate figure in Plates 22–25—namely that which indicates truly whether the red end of the spectrum is or is not darkened.

Scoring a test like this is very complex, but the following system appeared fair and was adopted. For each incorrect reading a mark of minus one (-1) was given. For each 'other' reading normal subjects were given a further mark of -1, but major defectives were given $-0{\cdot}5$, because an 'other reading' is altogether wrong for normal, but can be viewed as partly right for major defective subjects. The scores were totalled and rank orders were given to the four plates in each of the six sets. These sets were then examined to find whether any of the plates in them had any special virtues in discovering particular kinds of defectives, and generally the best two plates in each set were picked out for each class of subject, with the intention of selecting a small number of plates which would be consistently the best for all classes. The six sets of plates will be discussed in order, and in writing this analysis an effort has been made to reveal the important points clearly

without overwhelming the reader with a mass of numerical tables and statistical comparisons.

III. Ishihara's classification of defectives

Before proceeding to this analysis, it is convenient to mention the classification of colour-vision defectives given by Ishihara (2). This is as follows:

- Congenital colour blindness
  - Total colour blindness
    - Complete
    - Incomplete
  - Red-green colour blindness
    - Red blindness
      - Complete
      - Incomplete
    - Green blindness
      - Complete
      - Incomplete

It is a neat, logical classification, apart from the purely verbal peculiarity of the class of 'Incomplete total colour blindness', and would appeal strongly if it bore any close relation to the facts. However, to take one example, total colour blindness is a well-known Mendelian defective character, though very rare, but the 'incomplete' form of it given by Ishihara would seem to correspond most closely to a combination of red-green and yellow-blue weaknesses, as understood in this research. This general colour weakness is an extreme form of variation of normal colour vision, combining minor defects, and, far from being classified with total colour blindness, is allelomorphic to it and therefore should be its very opposite. It is not a sub-type of total colour blindness at all. The writer has, in a more extended research (8, 9), found absolutely no evidence that red-green blindness can be divided into 'red' and 'green' blindness. All so-called 'red' and 'green' blind subjects are red-green blind. The true distinction lies between protanopes, in whom the red end of the spectrum is greatly darkened, and deuteranopes, in whom all the spectrum is of normal brightness. These might be subdivided into 'moderate' and 'extreme', which suggests the 'incomplete' and 'complete' groupings of Ishihara. The next requirement of importance, which he overlooks entirely, is that red and green anomalous subjects must be added, and these could not be classed as 'incompletely red and green blind'. They are subjects who require different proportions of red and green in a mixture to match a standard yellow, according to the Rayleigh Equation, and make matches in this way which differ by very wide margins from those of normal people. They cannot be distinguished from protanopes and deuteranopes by Ishihara's test. Where Ishihara in his notes refers to 'red' and 'green' blind, these terms will therefore be assumed to correspond to 'protanope' and 'deuteranope', for the purposes of this article.

IV. Plates 2–5

According to Ishihara these plates should be read as follows:

Plate no.	Two	Three	Four	Five
Normal reading	8	6	29	57
Red-green blind reading	3	5	70	35

The seven groups of normal and minor defective subjects in this research (comprising the absolutely normal, the deviant and the colour weak), made on the average 0·78 error per subject on Plates 4 and 5 together, and 0·09 error on Plates 2 and 3. There were no significant average differences between any of the deviant or colour-weak groups and those who were absolutely normal. These four plates could not be used to distinguish between any minor defectives, but Plates 2 and 3 were about nine times more reliable than Plates 4 and 5 for all these subjects taken together. The explanation of this is probably to be found in

the complexity of the figures in Plates 4 and 5, which lend themselves to many interpretations.

For the green anomalous, however, Plates 4 and 5 were more reliable than Plates 2 and 3; for deviant deuteranopes Plates 2 and 3 were more reliable; for moderate deuteranopes Plates 3 and 5 were the best; for extreme deuteranopes the order for normal subjects held once more. There is therefore little or no consistency in the degrees of reliability of these four plates for green anomalous subjects and deuteranopes. On the average in these groups the negative score was −0·50 for the two better plates, whichever they were, per subject, and −0·85 for the two less reliable plates. There was, however, far too much overlap in the frequencies of various readings to use any of these plates to classify individual subjects, and it would be impossible to pick out two plates consistently reliable for all these types of defectives. Furthermore, those plates most reliable for normal subjects would not necessarily be the best for the major defectives.

For the red anomalous and extreme protanopes the order of reliability was the same as for normal subjects, but for moderate protanopes Plates 2 and 5 were better than Plates 3 and 4. Since the latter order is based on a very small number of false readings we may assume that the normal order would satisfy all the red anomalous and protanopes. The average negative score per subject for the two more reliable plates, taken together, whichever they were, was −0·23, while the score for the two less reliable plates taken together was −0·48.

It is true that the green anomalous subjects (and deviant and moderate deuteranopes to a less extent) show a tendency to be able to read the 'normal' figures as well as the 'colour blind' figures for Plates 2 and 3, and to be able to give numerous 'other' readings for Plates 4 and 5. This is not true of the red anomalous, moderate and extreme protanopes and extreme deuteranopes, all of whom are more consistent in reading only the 'colour blind' figures. Nevertheless, the test could not be used to classify any individual subject with a degree of certainty to be of any practical or theoretical value. It would be possible to say that a given subject, who made many alternative readings, was more likely to be green anomalous than a deviant, moderate or extreme deuteranope, and if he did not see the magenta figures in Plates 22–25, red anomaly (with darkened red) and protanopia might be excluded. This interpretation, however, would be far too uncertain to be worth anything to science. He might be red anomalous with red of normal brightness.

V. Plates 6–9

Ishihara says that these plates should be read as follows:

Plate no.	Six	Seven	Eight	Nine
Normal reading	5	3	15	74
Colour-blind reading	2	5	17	21

The order of reliability in these four plates for absolutely normal subjects is 8, 7, 6, 9. For all the minor defectives, however, it is 8, 6, 7, 9, but the differences are not great except for Plate 9. The average negative score per subject per plate for Plates 6–8 for all these seven classes of subjects together is −0·13, and for Plate 9 it is −0·45, which is more than three times as great. Undoubtedly Plate 9 is very unreliable, because it frequently suggests that a normal subject is defective, but it is quite incapable of discriminating between any groups of minor defectives and subjects who are absolutely normal. Collins (1) discusses this plate, and draws attention to Miles's statement that it is 'certainly the most sensitive

indicator of colour weakness that we possess'. After pointing out some of the alternative readings given for this plate, Collins adds the comment: 'It may be, of course, that this plate offers a very delicate test of colour weakness, and therefore is very effective in picking out colour defect of varying degree. But sometimes it was the only error the individual made not only in this test, but in a group of tests.' Undoubtedly, this plate is the most unreliable in its group for normal subjects, and, if it were assumed to be a very sensitive guide to small weaknesses, a large proportion of completely erroneous rejections of individuals would be made. The group of 174 minor defectives of all kinds made an average negative score of −0·39 on this plate, while the sixty-five absolutely normal subjects' average score was −0·48.

For the major defectives, however, we are again faced with the kind of difficulty found with the previous set. The more reliable plates for the green anomalous are 6 and 9; for deviant deuteranopes 6 and 7; for moderate deuteranopes 8 and 9; and for extreme deuteranopes 6 and 9. The average negative score per subject for the two more reliable plates, whichever they were, among all these subjects was −0·15, while for the two less reliable it was −0·44, which is almost three times as great.

For red anomalous subjects Plates 6 and 7 are very slightly better than Plates 8 and 9; for moderate protanopes Plates 7 and 8, and for extreme protanopes Plates 8 and 9 are the better pairs. The differences, however, are negligible, because the average negative score per subject for the two better plates, whichever they are, amounts to −0·10, while it is −0·23 for the less reliable pair, and this is not much more than twice as great, working with a very small number of errors and false readings.

In general, the verdict on these plates would be that Plates 6 and 7 might be chosen as most likely to do justice to normal and minor defective subjects, and these would not be open to serious criticism for the red anomalous, protanopes and deviant deuteranopes. For the green anomalous, however, and for moderate and extreme deuteranopes, Plate 9 would be preferred to Plate 7, while for moderate deuteranopes Plate 8 would also be preferred to Plate 6. We should therefore have considerable difficulty in choosing any two plates out of these four, to make a shortened series which would have any practical value. Again it is a general characteristic of the green anomalous, and to a less extent of deviant and moderate deuteranopes, to offer a number of 'normal' and 'other' readings in addition to the 'colour-blind' readings, but no consistent classification of individual subjects could be based on this criterion.

Finally, comparing the first two groups of plates, the fact that the first group show pink figures with green backgrounds and the second group show green figures whose backgrounds are pink, makes no difference to their validity or reliability for any type of colour-vision defect. This is true throughout the test. Some testers have thought that it should be harder for a deuteranope or green anomalous subject to see a green figure on a pink background than the reverse, and vice versa for the red anomalous and protanopes, and this idea is unfortunately fostered by the common view that they are 'green' and 'red' blind respectively. The facts do not support this expectation in the very least degree.

All eight plates are unfortunately constructed. On the whole, the single-figure plates (Plates 2, 3, 6 and 7) tend to be better for normal and minor defectives, whereas the double-figure plates (Plates 4, 5, 8 and 9) tend to be better for major defectives. All these plates are marred by the numerous possibilities for varieties of readings offered by the alternative

figures for normal and defective subjects. Too many subjects, both normal and defective, tend to be able to see both kinds of figures, and to combine parts of them in various ways, so that they are not clear-cut alternatives. This is a sad criticism because the figures are beautifully constructed and are masterpieces of colour printing.

VI. Plates 10–13

According to Ishihara these plates are read as follows:

Plate no.	Ten	Eleven	Twelve	Thirteen
Normal reading	2	6	97	45
Colour-blind reading	—	—	—	—

For normal, deviant and colour-weak subjects Plates 10, 11 and 13 are the best, and Plate 12 is the least reliable (although for green deviants there is almost no choice between the four). The average negative score per plate for the best three plates for all these subjects was −0·09, whereas the corresponding negative score for Plate 12 was −0·24, which is nearly three times as great. It would be difficult to choose between Plates 10, 11 and 13 for these subjects. The yellow and blue are slightly more consistently 'normal' than the red and green minor defectives in their readings.

For the green anomalous and deviant deuteranopes Plates 11 and 13 are the most reliable, whereas for moderate deuteranopes Plate 12 is very slightly the best, and for extreme deuteranopes Plates 10 and 11 are to be preferred. On the whole it would be satisfactory to use Plates 10 and 11 (or 13) and to omit Plate 12 for these subjects. The green anomalous again show a slight tendency to be able to see more alternatives than other subjects. For the red anomalous Plate 12 is again the least reliable, and it would be very difficult to choose between the remaining three, while this group of subjects tends to be more consistent in 'failure' than the green anomalous and deuteranopes. The result for protanopes is similar to that for the red anomalous. Number of errors is no guide whatever to the degree of defect.

VII. Plates 14–17

The readings for these plates are given by Ishihara as follows:

Plate no.	Fourteen	Fifteen	Sixteen	Seventeen
Normal reading	5	7	16	73
Colour-blind reading	—	—	—	—

The absolutely normal subjects, red deviants, red-green weak, yellow deviants and blue deviants all respond more consistently to Plates 14 and 15 than to Plates 16 and 17. For the green deviants and the yellow-blue weak subjects Plates 15 and 16 are more reliable than Plates 14 and 17. The negative scores for Plates 14 and 15 together for all these groups averaged −0·05 per subject, and −0·49 for Plates 16 and 17 together. For this group of subjects, therefore, Plates 14 and 15 might be chosen as the most generally reliable pair in this set of four.

For all the major defectives the reliability of these four plates is almost the same. No choice could be made between them. The green anomalous, however, again tend to be able to read the 'normal' figure fairly easily, but this criterion could not be used to distinguish them consistently. In general, therefore, Plates 14 and 15 can be chosen for the major defectives as well as for the normal group.

VIII. Plates 18–21

The plates in this group are read as follows, according to Ishihara:

Plate no.	Eighteen	Nineteen	Twenty	Twenty-one
Normal reading	—	—	—	—
Colour-blind reading	5	2	45	73

There is almost nothing to choose between these plates from the point of view of reliability for the groups of normal subjects. It is interesting, however, to see whether the yellow and blue are less able to read the figures than the red and green minor defectives, because, on the general hypothesis that they are legible to the red-green blind, this would be expected. Absolutely normal subjects gave an average negative score for these four plates taken together of −1·74; red and green minor defectives gave −0·96; yellow and blue minor defectives gave −1·37. The expectation mentioned above is therefore not supported by the facts, and no distinctions between minor defectives and absolutely normal subjects can be made by the use of these four plates.

For all major defective subjects Plates 18 and 19 are more reliable than Plates 20 and 21. The average negative score for Plates 18 and 19 together per subject was −0·51, whereas for Plates 20 and 21 it was −1·00. The difference is not very great. The plates are on the whole unreliable for all subjects. About 35% of subjects in the normal groups can read a figure on one or other of the plates, and about the same proportion of major defectives can give one or other plate a reading which is 'wrong' for them. As before, there is a tendency for the green anomalous, and to a lesser extent for the deviant and moderate deuteranopes to offer varied responses or no reading for these plates.

IX. Plates 22–25

These four plates are, according to Ishihara, read as follows:

Plate no.	Twenty-two	Twenty-three	Twenty-four	Twenty-five
Normal reading	26	42	35	96
Deuteranope reading	2 –	4 –	3 –	9 –
Protanope reading	– 6	– 2	– 5	– 6

For the normal and minor defective subjects these plates are all very reliable, the negative scores are negligible and there is nothing to choose between the plates for any of the groups of subjects concerned. In the sense that normal and minor defective subjects very rarely make any errors on any of these plates, they are among the best in the whole test.

For the green anomalous and deuteranopes there is nothing to choose in terms of order of reliability. The average negative scores per plate per subject are interesting, however, and are as follows: green anomalous −0·44; deviant deuteranopes −0·35; moderate deuteranopes −0·40; extreme deuteranopes −0·05. The plates tend to be less satisfactory for the less extreme types of subject.

Plates 22 and 23 are more reliable than Plates 24 and 25 for red anomalous subjects with red of normal brightness) and moderate protanopes, but for red anomalous with darkened red Plates 25, 23 and 24 are better than Plate 22, in that order, but for extreme protanopes Plates 23 and 24 are better than Plates 22 and 25. On the whole, therefore, Plate 23 proves to be the best for all these subjects. As explained before, however (10), all these plates are very unreliable for their real purpose, namely that of distinguishing darkened red.

X. Discussion and conclusions

It will now be useful to attempt to pick out any plates in the whole test which might be used to make a short and more reliable version of about six plates.

In the first group, Plates 2–5, reliability varies so much among the different classes of subjects that none can be chosen. In the second group, Plates 6–9, the same difficulty is met, and therefore all the plates with alternative figures to be read by major defectives are unsatisfactory and would be best omitted.

In the third group a good compromise might be reached by choosing Plates 10 and 11 as being more generally reliable than Plate 12, though Plate 13 might be substituted for Plate 11 without much danger. In the fourth group agreement might be reached by choosing Plates 14 and 15 as better than Plates 16 and 17, and in general, therefore, single-figure numbers are better than two-figure numbers, probably because they offer fewer possibilities for misinterpretation in form perception. One plate might be chosen from the fifth group, although these are all rather unreliable, and the choice would fall on Plate 18. Similarly, one might be selected from the sixth group, and it would be Plate 23.

In all, therefore, a shortened form of the test, which would be more reliable, could be made by taking Plates 10, 11, 14, 15, 18 and 23. To these it would be wise to add Plate 1 in order to have a 'joker' to catch malingerers and to show other subjects what to do in the test. This version of the test would tend to do less injustice to all classes of subjects than the 25-plate version, but it would still be of no use in distinguishing any particular type of defective, either major or minor, although many major defectives with darkened red could be picked out, with a low degree of reliability. The shortened test would have the disadvantage that the number of plates would be very small. They might easily be learned and known individually by subjects wishing to deceive the tester, even if presented in irregular order, and he would have but one-third of the number in the present full test material to exploit if in doubt about the result for any given subject. There are probably advantages in having as many as twenty-four critical plates, and those in which the major defectives can usually find an alternative reading are useful to give them confidence.

It must be agreed that the Ishihara Test achieved the essential purpose of its inventor very adequately, despite the many criticisms which may be made of it, and it is probably the best set of isochromatic plates available. That purpose was to cut out major red-green defectives in selecting candidates for employment on the Japanese railways. The shortened version published as the 5th edition, and discussed by Collins (1), consists of twelve plates and Plate 1 which is still the 'joker'. Plates 2, 3, 6, 9, 10, 11, 14, 15, 18, 19, 22 and 23 taken from the full test make up the shortened test of the 5th edition, in that order. Thus Ishihara has accepted four of the plates with alternative readings for the colour blind, including Plate 9, which is probably the least reliable of all. His choices of plates without alternative reading are the same as those chosen in this paper, namely those showing single-figure numbers. Of the plates legible to the colour blind he has chosen two, including Plate 18 which was preferred here; and of those intended to differentiate darkened red he has selected two, also including the one which proved best in this research. The 6-plate version advocated here would be decidedly better than this 12-plate version.

In order to indicate the numbers of 'colour-blind' readings which might be expected to amount to a 'failure' on the 6-plate version of the Ishihara test, Table 2 has been drawn up, and shows the frequencies of clear errors for each plate (exclusive of 'other' readings,

and always counted as errors from the point of view of normal colour vision) made by the subjects in the fifteen groups discussed in this paper. It also shows the average number of errors per subject, per plate, and the grand averages for the normal and major defective groups.

It is clear that the inclusion of Plate 18 makes the series less reliable for normal subjects, whose average errors would be reduced from 0·400 to 0·113 if it were excluded. This plate, however, makes a negligible difference for the major defectives. It is a useful plate to include, because of the advantage to the practical tester of having at least one plate on which the major defective may be expected to be able to read something, while the normal are usually unable. One extreme deuteranope who was tested in a variety of ways always refused to admit that he was in any way abnormal until he found he could read figures invisible to other people.

In spite of the inclusion of Plate 18 it is evident that there is a clear discontinuity between the frequency of definite errors made by the normal and the major defective

Table 2. *Frequencies of 'Colour-Blind' readings on the 6-plate version of the Ishihara Test, for fifteen groups of subjects*

Subjects		Plate numbers and frequency of errors						Average
No.	Type	10	11	14	15	18	23	errors
65	Absolutely normal	0	1	0	0	18	0	0·29
29	Red deviant	1	2	1	1	13	0	0·62
21	Green deviant	2	2	2	2	5	1	0·67
51	Red-green weak	1	7	0	1	15	0	0·47
9	Yellow deviant	0	0	0	0	4	0	0·44
9	Blue deviant	0	0	0	0	0	0	0·00
55	Yellow-blue weak	1	0	1	1	14	0	0·31
	Grand average errors for normal subjects							0·40
42	Green anomalous	25	31	29	30	30	9	3·67
20	Deviant deuteranope	17	18	19	18	18	15	5·25
21	Moderate deuteranope	16	16	16	16	16	10	4·29
14	Extreme deuteranope	14	14	14	14	13	14	5·92
7	Red anom. (normal red)	5	6	6	6	6	6	5·00
7	Red anom. (darkened red)	7	7	6	7	5	4	5·24
10	Moderate protanope	9	10	9	9	10	4	5·10
24	Extreme protanope	24	24	24	24	24	5	5·21
	Grand average for major defectives							4·97

groups on the 6-plate test. The latter make about twelve times as many errors as the former on the average, and there is no overlap in the frequency distributions. Three or four (or more) definite errors are therefore sufficient to fail major defectives, and subjects who make errors only on Plate 18 (by reading it) are not to be regarded as in any way defective. Green anomalous subjects cannot be detected individually.

The choice of Plates 10, 11, 14 and 15 agrees with Collins's (1) item-analysis of the Ishihara Test (5th edition), in which she found that these four plates were 'very significant'. 'The normal group shows a perfect pass in all four', she says, 'and the colour blinds almost a complete failure.' She found that the four plates with alternative readings for defectives (Plates 2, 3, 6 and 9), were less reliable for the colour-blind than for the normal subjects, and the present research agrees with her finding that the plates supposed to be illegible to normal people (Plates 18 and 19), were less reliable for normals than for the colour blind. Plates 22 and 23, however, she considered 'very satisfactory', a view with which it is very difficult to agree.

XI. Summary

In the course of researches on individual differences in colour vision, the 25-plate version of the Ishihara Test (Japanese Printing) was compared with tests based on the Rayleigh Equation, of 103 normal men and 136 normal women, 36 men and 6 women who were green anomalous, 48 men and 1 woman deuteranopes, 11 men and 1 woman who were red anomalous, and 30 men and 3 women protanopes.

The Ishihara test was divided into six groups of four plates in each group (excluding Plate 1, the 'joker'), and each group was considered separately. It was shown that the eight plates in which there is an alternative reading for major defectives were all unreliable, either for normal or for major defective subjects (or both). The single figure plates of the next eight, in which there is no alternative for major defectives, were more reliable than the double-figure plates. The plates intended to be legible to the colour blind were rather easily read by normal subjects. The plates which are intended to distinguish darkened red subjects were all unreliable.

A shortened form of the test consisting of Plates 10, 11, 14, 15, 18 and 23 (to which Plate 1, the 'joker', was added) was proposed, and it was shown that normal and minor defective subjects made about 0·40 error per subject and major defectives about 4·93 errors per subject for the six plates together.

No plates among the twenty-four were better than others for distinguishing any particular type of defect, and the test cannot be employed for this purpose, except that a proportion of darkened red subjects can be identified. The green anomalous tended to give more varied responses than other groups, but could not be identified individually. No degrees of difference in sensitivity among either normal, minor or major defectives could be measured by the numbers of errors made on the test, and yellow and blue minor defectives were not distinguishable. A warning is given in the introduction to Stilling's *Isochromatic Tables* (12), that the tester must resort to other and more efficient methods of measuring colour vision weaknesses, such as the anomaloscope, if he wishes to study subtypes and small differences. This is true of all isochromatic colour tests, wools, beads and lanterns.

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(*Manuscript received* 6 *January* 1950)

AN EXPERIMENT CORRELATING THE NATURE OF IMAGERY WITH PERFORMANCE ON A TEST OF REVERSAL OF PERSPECTIVE

By ROSEMARY A. GORDON

I. *Introduction* (pp. 63). II. *The Problem* (pp. 63–64). III. *Experimental procedure* (pp. 64–65). IV. *Results* (pp. 65–66). V. *Discussion* (pp. 66–67). VI. *Conclusion* (p. 67). *References* (p. 67).

I. Introduction

In an earlier investigation (1) it had been found that two different kinds of imagery processes existed, on the basis of which people could be divided into two contrasting groups. On the one hand, there were people whose imagery tended on the whole to be 'autonomous', that is to say, the images which they experienced were relatively independent of any volitional control that they might wish to exert. Hence table-legs might collapse, people change their appearance, gargoyles peer from behind curtains and the owner of such nightmarish pictures find himself unable to alter or dismiss them. The other group, in contrast, consisted of persons whose images appeared to be part of a more or less integrated functioning of personality so that the nature, appearance and disappearance of these images was under the conscious control of the subject. This differentiation of people into those with autonomous and those with controlled imagery was thought of as an extension of the work done by Jaensch on eidetic imagery.

The importance of this distinction was found to consist in the fact that the nature of the images differed between the autonomous and the controlled group, the former being more liable than the latter to produce stereotyped, that is rigid and change-resisting image contents.

II. The problem

As a next step, it appeared valuable to attempt to find some more objective criteria which might corroborate the differentiation of imagery processes and serve perhaps at a later date as an additional method for the assessment of controlled and autonomous image types. In the search for this corroborating evidence the work on reversal of perspectives suggested itself. For one can argue with some justification that perceptual and imagery processes are closely interlinked and interdependent, exerting a mutual influence on one another. Few would question the view that images are to a great extent built up and dependent upon the data derived from perception; perception itself, on the other hand, as has been shown by the experiments of the Gestalt Psychologists, F. C. Bartlett (2) and others, is determined not only by the stimulus or even the general extra-psychic situation, but also by such intra-psychic forces as the needs, interests, emotions and moods of the perceiver. But here, it would seem, the general fund of images possessed should also be included. Mr Chiang Yee's perception of London (3) is undoubtedly affected by his particular collection of images, of which Chinese paintings—and through these a more or less 'Chinese' perception of the external world—must constitute a large proportion. Furthermore, experiments demonstrating the difficulty subjects might experience in distinguishing a true perception from an image when they have been prepared to expect a relatively faint

sensation may serve as a further indication that perception and imagery function in similar ways and are closely related processes.

Perception, however, is a process which lends itself very much better than does imagery to objective methods of investigation. It would therefore be valuable if an examination of imagery relying predominantly on introspective techniques could be extended and confirmed by results from experiments on perception.

The above considerations led to the question whether any correlation could be found between the type of imagery possessed by a person and the characteristics of his rate of reversal. Reversal of perspective is above all a perceptual process, but since the stimulus itself remains constant throughout, changes in the perception must be the result of psychophysical mechanisms in the individual.

Consequently, the following problem suggested itself: Do subjects with autonomous imagery differ significantly from subjects with controlled imagery in the rate of reversal per unit time on a test of reversal of perspectives? If they do, one or both of the following differences might be observed:

(*a*) One of the two image types may show a consistently higher or consistently lower rate of reversals.

(*b*) One of the two image types may be more capable of varying the rate of reversals and so display a greater volitional control over this particular perceptual process.

The second point of difference seemed to be particularly apposite, especially in view of the aforementioned results from the imagery test. These might lead one on to the hypothesis that if any difference can be discovered between autonomous and controlled subjects on a reversal of perspectives test, this difference will consist in the fact that subjects with controlled imagery are better adapted to exert volitional control on their rates of reversal of perspectives than are subjects with autonomous imagery. The following experiment was therefore set up to test out this hypothesis.

III. Experimental procedure

Forty-two male neurotic patients from the Mill Hill Emergency Hospital and the Sutton Emergency Hospital were tested, each individually, as follows.

The phenomenon of the reversal of perspective was described to each patient, the normality of the phenomenon being particularly emphasized. A card on which was drawn the reversible box pattern was then presented to him. He was instructed to look at the card for one whole minute, the examiner keeping the time by means of a stop-watch, and to tap the table with a pencil that was provided each time he noted a change in his perception of the drawing. (In the absence of a smoked drum this rather primitive method of notification of reversals had to be resorted to, but it is doubtful that this has affected the results in any great measure.) The examiner noted the number of reversals notified by the subjects during one minute when no instructions had been given to him as regards the desirability to have either few or many reversals. The rate of reversal thus obtained was regarded as the subject's natural or normal rate.

After a few encouraging words, the subject was next instructed to attempt to increase the number of reversals per minute as much as he could and to try and obtain as many changes of perspective as possible.

Lastly, the patient was told that as a contrast he was now required to reduce the number of reversals per minute as much as was in his power.

After the completion of this part of the experiment the second part of the test was introduced; this was designed to establish what kind of imagery the patient possessed. The test procedure here was more or less identical with the one used in the previous investigation of controlled and autonomous imagery processes. First of all the meaning of 'image' was described to the patient and often illustrated. (Seeing each patient individually and conducting this part of the examination rather like a standardized interview it was possible to make quite sure that the patient understood what was wanted of him.) Next he was questioned about his particular type of imagery and this was discussed in some detail. Lastly, he was asked to image the following scenes:

(1) A car standing in front of a garden gate.
(2) The same car but in a different colour than that seen at first.
(3) The same car lying upside down.
(4) The same car back on its four wheels.
(5) The car running along the road.
(6) The car climbing up a very steep hill.
(7) Climbing across the top of the hill.
(8) Getting out of control and crashing through a house.
(9) The same car running along the road with a handsome couple inside.
(10) Crossing a bridge and falling into the stream below.
(11) The same car all old and dismantled standing in a car-cemetery.

The patient's success in imaging these various scenes was used as a check on the information he had given about his ability to control his images.

Throughout this examination it was stressed that this investigation was concerned with quite normal processes and was independent of the fact that the subject happened to be a patient in a Neurosis Centre.

IV. RESULTS

Of the forty-two patients tested, twenty were discovered to possess autonomous imagery, while twenty-two had controlled imagery. (Only patients who were thought to have controlled imagery on the basis of the information they provided in the interview and who were subsequently found capable of imaging every one of the eleven car scenes presented to them were classified as 'controlled' image types.)

Tables 1 and 2 set out the results obtained, showing the number of reversals per minute, given the three different instructions.

These figures were subjected to statistical analysis, using the t formula.

No significant differences were found between the two experimental groups as regards the tendency to have either a high or low frequency of reversals. The means for the reversals in response to the three different instructions are given in Table 3. Comparing the two groups for their fast speed, in which case the difference in their mean number of reversals was greatest, t was found to be 1·244, so that P was greater than 0·2. Consequently, it can be assumed that the two groups do not differ in respect of any of the three absolute reversal rates.

However, the groups did differ significantly in their ability to vary the rate of reversals. Thus, when comparing their ability to change from a normal to a fast rate of reversals, t—calculated on the basis of the difference in numbers of reversals for 'normal' and 'fast' speeds—was found to be 2·003, P being therefore less than 0·05, while in the case of a change

from fast to slow t was found to be 2·281 and P therefore a little more than 0·02, the autonomous group proving itself in each of these two cases significantly less capable of adjusting the rate of reversal to the new instructions.

Table 1. *Patients with autonomous imagery*

Subject no.	Normal speed	Fast speed	Slow speed
1	16	17	14
2	16	13	9
3	21	10	34
4	3	4	4
5	20	15	3
6	5	5	4
7	4	9	3
8	10	38	28
9	4	8	3
10	7	7	0
11	7	15	9
12	15	24	14
13	8	11	13
14	17	13	8
15	8	11	8
16	1	3	0
17	6	4	3
18	16	13	3
19	4	10	5
20	13	14	8

Table 2. *Patients with controlled imagery*

Subject no.	Normal speed	Fast speed	Slow speed
1	3	11	7
2	9	44	3
3	11	22	3
4	15	28	6
5	15	14	6
6	21	59	26
7	16	24	12
8	8	10	1
9	7	22	4
10	2	13	2
11	9	19	5
12	11	27	7
13	12	22	5
14	5	2	1
15	13	10	11
16	5	13	2
17	16	12	8
18	18	22	3
19	23	41	20
20	8	18	8
21	6	6	2
22	1	4	1

Table 3

	Autonomous	Controlled
Group means for normal speed	10·05	10·64
Group means for fast speed	12·20	20·17
Group means for slow speed	8·74	6·40

V. Discussion

This experiment may be used to advance tentatively the hypothesis that stereotypy—that is the tendency to resist change—is an attribute which may characterize to a greater or lesser extent a number of a person's mental traits and processes. Perception and imagery being closely related, as was postulated earlier, are particularly liable to show similarity

n respect of stereotypy so that the examination of one of these processes in any particular ubject tends to lead one to a relatively reliable inference about the degree of stereotypy ossessed by the other.

However, it would be premature to draw any inferences from these results on the further uestion of constitutional type. Neither imagery nor the tendency to reverse perspectives as yet been shown to be unrelated to, and independent of, other mental and emotional raits which may be changing, mutable and subject to the impact of environmental forces. 'ameron (4), for instance, has reported that depressive patients give a more normal and egular rate of reversals as their mental health improves. Hence, though not unrelated to aensch's work on eidetic imagery (5), this investigation does not claim to support or refute is thesis as regards constitutional types.

VI. CONCLUSION

1. The results from this experiment suggest that people can be divided into two groups n the basis of their performance on a rate of reversal of perspective test, and that this ifferentiation of the experimental sample runs parallel to a differentiation according to he presence of autonomous and controlled imagery. Hence there appears to be a con- exion between the type of imagery a person possesses and the nature of his performance n a rate of reversal test.

2. A significant correlation was found between type of imagery of a person and the bility to control the rate of reversal, so that the subject whose imagery is relatively ontrolled is capable of exerting more volition in relation to rate of reversal than lies ithin the ability of the autonomous imagery type. No correlation, however, was found etween imagery and the actual rate of reversal.

3. These results therefore corroborate the evidence from a previous investigation which uggested that individuals can be distinguished according to whether their imagery pro- esses show a certain amount of autonomy or whether they are always well under volitional ontrol. For it appears that the difference which these two groups exhibit as regards magery is associated with differences in perceptual processes also.

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(*Manuscript received* 20 *February* 1950)

NOVELTY AND CURIOSITY AS DETERMINANTS OF EXPLORATORY BEHAVIOUR[1]

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I. *The importance of novelty* (pp. 68–69). II. *Historical* (pp. 69–71). III. *The problem of novelty* (pp. 71–72). IV. *A theory of curiosity* (pp. 72–75). V. *Experiment* (pp. 75–79). VI. *Summary* (p. 79). *References* (pp. 79–80).

I. The importance of novelty

Psychology has so far had surprisingly little to say about stimuli which influence behaviour simply because they are new. Stimuli which owe their potency to the fact that they are not new, i.e. that they have been experienced before in certain circumstances, have given rise to the vast corpus of observations and generalizations that go to make up learning theory. But the everyday activities of both men and animals seem to attest the importance of novelty as well as familiarity in features of the environment, and novelty may, in fact, hold the key to our understanding some of the more complex levels of motivation in the human being. Few human impulses are more inexorable than the urge to escape from monotony and boredom to some new form of stimulation, as the industrial psychologist and even the psychiatrist have so frequently reason to note. An active striving to encounter new experiences, and to assimilate and understand them when encountered, underlies a huge variety of activities highly esteemed by society, from those of the scientist, the artist and the philosopher to those of the polar explorer and the connoisseur of wines. 'The instinct of curiosity', says McDougall (13), 'is at the base of many of man's most splendid achievements, for rooted in it are his speculative and scientific tendencies.'

Reactions to or towards novelty are met with in one guise after another when the problem of interest is examined (cf. Berlyne (1)); indeed, in one sense, 'interest' is used more or less as a synonym for 'curiosity'. And if one turns from interest to a closely related topic, one finds novelty figuring prominently among the qualities that can give stimuli the power to attract attention, a factor whose study has been unduly neglected (cf. Berlyne (2)). There would thus appear to be few aspects of human behaviour which might not be illuminated by some research into the effects of novel stimuli, including the question of curiosity, and yet such investigation has been remarkably lacking hitherto. One probable reason for this lack is that the relevant phenomena are so enormously complicated in man that observations of human beings cannot advance far without being supplemented by studies of the prototypes of human curiosity and response to novelty in lower animals. And the most suitable animal for such studies is undoubtedly the rat. Apart from the well-known and frequently reiterated virtues of the rat as a laboratory subject, research into his exploratory behaviour may enable us to knit the functioning of curiosity into the large body of systematized knowledge that has been acquired from rat experiments and has been shown to be pertinent in the highest degree to human psychology.

[1] The latter half of this paper, under the title 'Novelty as a Problem in Behaviour Theory', was read to a meeting of the Scottish Branch of the British Psychological Society at St Andrews on 6 May 1950.

Now, a consideration of the role of novelty in the behaviour of the rat leads us to much the same conclusions as with the human being: any account of rat psychology which neglects to mention and explain curiosity and response to novelty must be seriously deficient, and yet very little has actually been done to remedy such a deficiency. One of the first facts learnt from even a few minutes' observation of rats is that they are highly inquisitive. Dashiell (3) and Nissen (18) have demonstrated the existence of an 'exploratory drive'; it is strong enough to induce a rat to cross an electrified grid in order to examine an unfamiliar environment. But next to nothing has been done to analyse the working of this extremely important form of motivation. What precisely are the adequate stimuli arousing it? What is the nature of the reactions it elicits? How does it come to be satiated? These, and a host of other questions concerning the laws of curiosity, remain unanswered, and a large body of experimental data will, no doubt, have to be painstakingly accumulated before they can be elucidated.

II. Historical

If experimental studies of curiosity have been scanty, theoretical treatments of the question have been slightly less meagre. And before going on to report the experiment with which this paper is concerned and the hypotheses it was designed to test, it may be helpful to consider briefly the principal theories that have so far been propounded.

(1) *Shand.* According to Shand ((21), pp. 438ff.), curiosity is a 'primary emotion', consisting of 'a single impulse to know, instinctively governing and sustaining the attention, and evoking those bodily movements which will enable us to gain a fuller acquaintance with its object'. But while the end of curiosity and the stimuli arousing it are innately determined, some, at least, of the actions subserving it in the human being are acquired. Thus, apart from talk about instincts 'innately directed' towards certain ends, which is too apt to lead to confused thought for contemporary taste, we have a conception which may be reconciled with modern learning theory and bears distinct resemblances to the view we shall outline later.

Also pertinent is Shand's account of surprise. There are two varieties of this emotion: 'sensational surprise' is a reaction to 'a jar or conflict...between a present sensation and the memory of preceding sensations', while in 'cognitive surprise', the conflict is between a present cognition and the memory of preceding cognitions'. Wonder, the 'source of the love of knowledge', contains both curiosity and cognitive surprise. It is aroused by anything extraordinary and difficult to understand.

(2) *McDougall.* Curiosity, with its corresponding emotion of 'wonder', is one of McDougall's list of instincts ((13, 14): '...its impulse is to approach and examine more closely the object that excites it.... The native excitant of the instinct would seem to be any object similar to, yet perceptibly different from, familiar objects habitually noticed.... It is...not easy to distinguish in general terms between the excitants of curiosity and fear...the two instincts, with their opposed impulses of approach and retreat, are apt to be excited in animals and very young children in rapid alternation, and simultaneously in ourselves' ((13), pp. 57–8).[1]

This treatment of the problem, with all its begged questions, is vulnerable to all the obloquy for which McDougall's doctrine of instincts was in the 1920's and 1930's such a popular target. But his description of the 'native excitant' or adequate stimulus of

[1] Quoted by permission of the publishers.

curiosity and his indication of a close link between curiosity and fear are extremely important and will be discussed later.

(3) *Freud.* The impulse for knowledge and investigation (*der Wiss- oder Forschertrieb*) is attributed by Freud ((4), pp. 594–5) 'on the one hand, to a sublimated form of acquisition (*Bemächtigung*)', while 'on the other hand, the energy with which it works comes from the looking impulse (*Schaulust*, scoptophilia)'. The impulse to acquire is derived from the desire to incorporate external objects, which characterizes the oral stage of psycho-sexual development, and the looking impulse, which underlies the perversion known as voyeurism, is one of the partial impulses contained in the libido. Curiosity is therefore sexual in origin; 'indeed' says Freud, 'curiosity may perhaps first be awakened by sexual problems'.

This may well be a most valuable contribution to our understanding of the development of curiosity in the individual human being, but it can hardly, for a number of reasons, be considered a complete explanation of the origins and functioning of curiosity:

(*a*) There is no lack of evidence that curiosity is not first awakened by sexual problems in the way that Freud meant. The fact that he goes on to speak of the question 'where do children come from?' as the first problem shows that he had in mind such curiosity as makes use of language. But many observers of early childhood (e.g. Piaget (20), p. 126) report intensive curiosity and investigatory activity long before the advent of speech: the infant is busily engaged in exploring and even experimenting with his environment, thereby acquainting himself with the diversity of the phenomena it contains and the connexions between them. If he had not already equipped himself in this way with exploratory techniques and habits, he could not put them at the disposal of his sexual curiosity later on.

(*b*) Behaviour analogous to that characterizing human curiosity is found in the lower mammals. Although many equivalents of psycho-analytic processes and mechanisms have been found in the rat and other animals, the Freudian theory of psycho-sexual development, which depends so thoroughly on verbal and imaginal symbols, can hardly be applied to infra-human species.

(*c*) A tendency to explore novel aspects of an animal's surroundings is so essential biologically that it is bound to have had an earlier source than the sublimation of libidinal impulses due to their repression at the behest of human society.

(4) *Holt.* For Holt ((8), p. 216), 'exploration' is 'random movement with adience sustained'. According to his principle of adience, there is an innate tendency to approach any stimulus, although some cause injurious overstimulation and thus set up avoidance responses. When an adient movement has been elicited, the organism is attracted to the surface of the object concerned. 'And within this locus the random movements are constrained to 'explore' the surface of the object; thus chain reflexes are developed (and fixed as neurograms) which, even when the object is absent, will re-create its surface contour.'

This is an ingenious attempt to face important questions which many other neurologically minded psychologists have ignored, but it cannot be regarded as satisfactory:

(*a*) Holt's universal and innate adience to all stimuli has not been experimentally demonstrated.

(*b*) No explanation is offered of why certain stimuli attract curiosity more than others.

(*c*) No explanation is offered of how curiosity comes to be satiated.

(5) *Taylor.* A recent theory of exploratory activity proposed by Taylor (23) points out that an animal will find a repertoire of 'slick responses to every aspect of his environment'

helpful in practically everything he does. A 'generalized habit' of learning 'to get about easily and quickly in any new environment' will therefore grow up, and this need to explore will function as a secondary drive. But stimuli which have frequently been associated with need-reduction are more likely than others to attract exploration, and behaviour-sequences leading to them will undergo secondary reinforcement.[1]

III. The problem of novelty

This review of theories of curiosity reveals that several different mechanisms, motives and adequate stimuli have been alleged by different writers to call forth exploratory behaviour. And these alternative accounts are by no means necessarily incompatible; it is quite likely that superficially similar forms of exploratory behaviour may be derived from different sources at different times or even at once.

For example, there are good theoretical and empirical grounds for believing that an organism can and will learn particular investigatory response-sequences which satisfy particular motives, whether we think of the laboratory-sophisticated rat seeking the goal-box of a maze or the detective searching systematically for clues. It is even probable that exploratory habits found useful in one situation may generalize to a large variety of analogous situations, as Taylor (23) asserts (and as Miller and Dollard (16) have shown to be the case with imitation).

But besides exploration motivated and learned in this manner (which we may term *instrumental exploration*) there seems to be exploration evoked by novelty. Taylor claims that the exploratory drive is 'increased in strength by unfamiliar surroundings'. But what is there about unfamiliarity itself that could evoke a particular reaction? The role of novelty presents a difficult problem: if there are responses which new stimuli elicit but old stimuli do not elicit, these responses must be due to some quality which all new stimuli possess and all old stimuli lack. But, apart from the purely negative fact of not having occurred before, what quality is there that characterizes novel stimuli and novel stimuli only?

In the case of stimuli which influence behaviour because of the organism's previous experience of them, there is no such problem: these stimuli or similar ones have all had the opportunity in the past to acquire the power to evoke learned responses. Nor is there the same difficulty with the stimuli of innate reflexes: they have in common certain physico-chemical properties which affect receptors and the nervous system in certain ways. But novel stimuli have no physico-chemical properties to differentiate them from others; being new, they cannot owe their effects to recognition. How therefore is the difference between the familiar and the unfamiliar communicated to the nervous system? How does the organism distinguish between old and new and respond differentially?

To this problem there would appear to be two possible solutions:

(1) It may be that *not all* novel stimuli are effective, but only those which are familiar in some respects and novel in others—i.e. stimuli such as Shand believed responsible for 'surprise' and McDougall for 'curiosity'. This presumably means that the familiar aspects set up some sort of expectation ('fractional anticipatory response' (16), 'preparatory set' (17),

[1] Pavlov (19) mentions the 'investigatory (or "what-is-it?") reflex'. But, although he believes that human 'inquisitiveness, the parent of...scientific method' has grown out of it, this can scarcely be numbered among these theories of curiosity. The adequate stimulus is not novelty, in any sense resembling those under discussion here, but any sudden change in the environment. The response is limited to orientation of receptor organs, and so it cannot account for the enormous variability of exploratory activity.

etc.) with which the perception of the novel feature conflicts. Such a conflict has been stated by Isaacs (11) to be the root of our conception of truth and falsity.

Now, in the adult human being it would indeed appear that curiosity is most often aroused not by just any novel perception but by some unexpected feature in familiar material, by something that is in some degree similar and in some degree dissimilar to what is well known to him. A sideshow in a fair-ground will attract more patrons if it exhibits a two-headed but otherwise normal man or a bearded but otherwise undistinguished lady than if it offers a collection of beetles or birds' eggs, all of which may be completely new to those who inspect them.

In view of what will later be said about the relation between curiosity and fear, it is interesting to remember Hebb's findings in his studies of fear in chimpanzees (6). Expression of intense alarm was produced by such stimuli as an anaesthetized ape, a model of a dismembered ape, etc.; Hebb comes to the conclusion that some strange variation in something familiar can excite fear, and he attributes the effect to a disruption of some habitual cerebral process.

But, without denying the existence of exploration induced by the unexpected (which we may term *surprise-exploration*), we shall provisionally assume that there is another type of exploration instigated by novelty pure and simple. Our reasons are as follows:

(*a*) To a human infant, very little is familiar. If the adequate stimulus for exploration is not novelty alone, but a combination of novelty and familiarity, it is difficult to see how the intensive investigatory activity of early years, which familiarizes the child with his surroundings, could occur.

(*b*) In the experiment to be described, the stimuli used—a wooden cube and a cardboard ring—do not appear to resemble anything the rats are likely to have previously encountered, let alone encountered frequently. Moreover, both cube and ring excited curiosity to a similar extent. It is highly unlikely that two stimuli selected at random would both have much the same degree of novelty in familiarity.

However, since the animals were not observed from birth, and since, in any case, there is no knowing what degree of resemblance to known objects is required, one cannot be sure.

If, as we assume, there is exploration in response to all novelty, we must resort to the second solution to the problem of novelty:

(2) The reaction may be one that *all* stimuli originally evoke, but which disappears (becomes habituated) as the organism becomes familiar with them. The crucial difference between novel and familiar stimuli that we have been seeking would then be that the former have not had time to lose their curiosity-inducing effect.

But, whatever form of novelty may ultimately turn out to be involved—whether all novelty or only the unexpected or some other form—the most urgent task would seem to be to demonstrate the influence of novelty on exploratory behaviour and to ascertain the manner of its functioning.

IV. A theory of curiosity

We are now in a position to formulate the theory of curiosity that our experiment is to test. It consists of two postulates from which, when combined with the postulates of Hull's behaviour theory, three corollaries describing the functioning of exploratory behaviour may be deduced. We shall go on to describe an experiment which will verify or falsify four predictions following from our postulates and corollaries.

To some readers, what follows may seem insufficiently formal. To others, it may seem a trite and pompous re-statement of common knowledge. But more research must be carried out before the objections of the former can be removed, while to the latter we may point out that before common knowledge becomes scientific knowledge, it must be couched in a form which will enable it to be rigorously verified and related to general principles, derived from studies of a great variety of other phenomena.

Postulate 1. When a novel stimulus affects an organism's receptors, there will occur a drive-stimulus-producing response ($R \rightsquigarrow S_D$) (which we shall call 'curiosity')

Comments

(*a*) This postulate is put forward in this provisional form with several reservations as discussed above:

(i) It is assumed that any novel stimulus is involved, but it may concern only certain forms of novelty.

(ii) It is assumed that the curiosity-response is innate, but it may be acquired.

(iii) By 'organism' is meant, for the time being, any mammalian organism.

(*b*) We must now consider the question of the relations between curiosity and fear. McDougall (13) states that the two may be aroused by similar excitants. More recently, Whiting & Mowrer have, in the course of one of their experiments, made some observations pointing in the same direction:

> Preliminary observations showed that if animals were placed in the maze without prior habituation, they showed considerable anxiety. This was first indicated by great cautiousness of movement and excessive urination and defaecation. Later there was a great period of feverish exploration, during which animals ignored food even though they had not eaten for 24–36 hours. These observations suggest that 'curiosity' is perhaps more closely related to anxiety than is ordinarily supposed ((24), p. 236).

This impression is certainly gained from watching rats casually. The same sort of stimulus, namely anything novel, seems to excite fear or curiosity, and often an animal appears to be wavering between the two.

Now, curiosity and fear are alike in being drives motivating the organism to put an end to them, in being aroused by external stimuli, and in being aroused in particular by the novel and the unexpected. But the behaviour associated with curiosity and fear differ markedly: the one brings about approach and the other withdrawal. If novel stimuli set up a state of tension with consequent random behaviour in an organism, two classes of response-sequence are likely to be followed by tension-reduction. One is approach and exploration and the other is withdrawal until the stimulus is out of range. An individual's proneness to one or the other may depend on temperament and experience, but on the whole it may well be that withdrawal is more effective with *stronger* stimuli (since approach may mean danger) and exploration with *weaker* stimuli (since these often represent food and other goal-objects). The difference between curiosity and fear may therefore be one of intensity only. But this question must be postponed, probably till more is known about the physiological concomitants of both.

(*c*) It may be worth noting that our hypothesis that all stimuli originally set up a state of drive (in experiential language, 'are painful') receives support from psycho-analytic sources. Freud (5) asserts that external objects, apart from those which are pleasurable and therefore introjected, come to be regarded in the child as forming a hostile external

world. 'Thus at the very beginning, the external world, objects, and that which was hated were one and the same thing' (p. 79). The view that during the first few weeks of life all stimuli are painful is fundamental to Melanie Klein's conception of the infantile mentality (12).[1]

Postulate II. As a curiosity-arousing stimulus continues to affect an organism's receptors, curiosity will diminish

Comments

(*a*) It is provisionally assumed that the habituation of the curiosity-response is a process of inhibition, similar to the inhibition of learned responses according to Hull's account. 'Negative adaptation' or 'habituation' of innate responses is of course a well-known phenomenon (see (7), pp. 105ff).

(*b*) We may note, with reference to the probable affinity between curiosity and fear, that Miller and his associates have observed behaviour in rats which seems to mean extinction of fear-responses by exposure to a fear-producing stimulus ((15), p. 438). Shaw has even attempted to explain the relief of anxiety in the psycho-analytic session by a similar process of extinction (22).

The following corollaries can now be deduced from these postulates plus those of the Hullian system.

Corollary i. An organism will learn to respond to a curiosity-arousing stimulus with activity which will increase stimulation by it. (*We shall call such activity 'exploration'.*) (Our Postulate II and Hull's Postulate III in *Behavior Postulates and Corollaries* (10), replacing Postulate 4 in *Principles of Behavior* (9).)

This follows from Hull's principle of reinforcement (a form of the 'law of effect'), according to which any response which is closely followed by drive-reduction (G) will tend to be repeated. Since stimulation by the curiosity-arousing stimulus leads to a decrease in the curiosity-response (and therefore in the curiosity drive-stimulus), activity increasing such stimulation will be reinforced. The organism will tend to respond to curiosity in future with such activity. 'Exploration' includes McDougall's 'approach to the object' and 'closer examination of the object by one or more channels of sense' ((14), p. 143). It will, of course, only occur if there is not simultaneously evoked an incompatible response with a higher momentary effective reaction-potential (${}_S\bar{E}_R$) (e.g. withdrawal through fear).

Corollary ii. After a time, exploration will cease. (Our Postulate II and Hull's Postulate X in *Behavior Postulates and Corollaries* (10), replacing Postulate 8 in *Principles of Behavior* (9).)

This follows from the Mowrer-Miller hypothesis: as a response continues or is repeated there accumulates a motivation towards its cessation ('reactive inhibition' or I_R). The curiosity-response will therefore undergo increasing reactive inhibition until it falls below a reaction-threshold (${}_SL_R$).

A particular exploratory response may, of course, succumb to reactive inhibition before curiosity ceases and may be succeeded by another, which no doubt explains why exploratory activity often takes the form of a series of varied responses. Similarly, the curiosity pertaining to one part or aspect of the unfamiliar object may be satiated, so that exploration may be directed to another part or aspect and the object investigated from one angle after another (Holt's 'random movement with adience sustained').

[1] Yet another approach, this time through neurological considerations, has since led a writer to the conclusion that *all* stimuli exciting the central nervous system are to some extent drive-inducing. See Wolpe (25).

Corollary iii. After a further lapse of time, if the curiosity-arousing stimulus is again affecting the organism's receptors, there will be a second spell of exploration, but this time there will be less exploration than during the first spell; such spells of exploration will recur at intervals, until exploration of the stimulus in question ceases altogether. (Our Postulate II and Hull's Postulate X and corollary ix in *Behavior Postulates and Corollaries* (10), replacing Postulates 8 and 9 in *Principles of Behavior* (9).)

This follows from Hull's two-factor theory of inhibition. All prolonged or repeated responses are subject to (*a*) 'reactive inhibition' (I_R), a factor dissipating with time, which explains why extinguished responses have intermittent phases of spontaneous recovery; (*b*) 'conditioned inhibition' ($_SI_R$) which is a learned and therefore permanent factor; its gradual accumulation explains why spontaneous recovery decreases with each successive phase, until it ceases altogether.

Both the curiosity-response and individual exploratory responses will undergo these two sorts of inhibition. They will therefore show spontaneous recovery at intervals, diminishing with each recovery until they fall below a 'reaction-threshold' ($_SL_R$) and are no longer manifested.

V. Experiment

Apparatus

The '*experimental box*' had an interior 25 in. long by 11 in. wide by 8 in. high. Its walls were lined with white paper, the floor consisted of a sheet of zinc; there was a roof of wire-netting through which the rats could be observed. A portion of the netting, 11 × 10 in., at one end of the box formed a hinged door, through which the rats were put into the box and taken out.

The 'stimuli' consisted of three 'cubes' (unpainted wooden cubes, 1 in. per side) and three 'rings' (hollow cylinders of brown cardboard, 1 in. in diameter and 1 in. in height, covered on the outside with black paper).

Subjects

The subjects were twelve male albino rats of the Wistar strain, aged approx. 90 days at the beginning of the experiment. They were divided randomly into an Experimental Group of six and a Control Group of six, each group being housed in one cage. Before each experimental session they were allowed to eat and drink to satiety.

Procedure

(*a*) *Experimental Group* (*six rats*)

Group habituation. All six rats were placed together in the empty experimental box for two hours. Each rat in turn then underwent an experimental session of sixty-five minutes made up as follows:

(i) *Solitary habituation.* Twenty minutes in the empty experimental box. This was to habituate the animal still further to the experimental conditions and to satiate curiosity directed to the box itself.

(ii) Ten minutes in the home cage with the other members of the group.

(iii) *Phase I: three similar* (*novel*) *stimuli.*. Five minutes in the box, where there were three similar stimuli (placed in a row across the floor, with 3 in. between neighbouring stimuli and at a distance of 11 in. from the end at which the rat entered). For three rats

(the Cube Sub-Group) they were three cubes, and for the other three (the Ring Sub-Group) they were three rings.

(iv) Ten minutes in the home cage.

(v) *Phase II: three similar (familiar) stimuli.* Five minutes in the box, containing the same three stimuli as in Phase 1.

(vi) Ten minutes in the home cage.

(vii) *Phase III: two similar (familiar) stimuli and one odd (novel) stimulus.* Five minutes in the box, where one of the stimuli present in Phases I and II had been replaced by a novel stimulus—i.e. for the Cube Sub-Group, one cube had been replaced by a ring, and for the Ring Sub-Group, one ring had been replaced by a cube. For one rat of each sub-group the novel stimulus was on the left, in the middle and on the right respectively, to exclude the effects of positional preferences.

In Phases I, II and III the time spent exploring (i.e. sniffing at) each stimulus was recorded to the nearest second by a three-pen chronograph, controlled by push-buttons which the experimenter operated. If there was no exploration in the first five minutes of Phase III, the rat was left in the box for a further five minutes. But this occurred only in the case of one subject (rat D).

(*b*) *Control Group (six rats)*

(The procedure for this group was equivalent to that of the Experimental Group with Phases I and II omitted and Phase III repeated.)

Group habituation. Two hours, as for the Experimental Group. Each rat in turn then underwent an experimental session of fifty minutes made up as follows:

(i) *Solitary habituation.* Twenty minutes as for Experimental Group.

(ii) Ten minutes in the home cage.

(iii) *Phase I: two similar (novel) stimuli and one odd (novel) stimulus.* Five minutes in the experimental box, where there were two similar and one odd stimuli, arranged as for Experimental Group. For three rats (controls for Cube Sub-Group) there were two cubes and a ring, and for the other three (controls for Ring Sub-Group) there were two rings and a cube. The odd stimulus was in a different position for each rat of each sub-group, as in Phase III, Experimental Group.

(iv) Ten minutes in the home cage.

(v) *Phase II: Two similar (familiar) stimuli and one odd (familiar) stimulus.* Five minutes in the box with the same stimuli as in Phase I.

Exploration of each stimulus in Phases I and II was timed as in the case of the Experimental Group.

Predictions

In Phase III the Experimental Group face one novel stimulus and two which they have already (Phases I and II) had the opportunity to explore. They should therefore, if our Postulate II is true, have more curiosity aroused by the former than by the latter and thus spend more time exploring it.

The Control Group (Phase I) have not had a prior opportunity to familiarize themselves with the two similar stimuli, and so their behaviour will show whether the above effect is really due to the novelty of the odd stimulus.

Similarly, in Phase III, Experimental Group, the two similar stimuli are familiar,

whereas in Phase I, Control Group, they are novel. If the same postulate is true, the Control Group should spend more time exploring them than the Experimental Group.

If our corollaries are correct, there should be some exploration in Phase II (both groups), when the same stimuli are presented as were explored in Phase I, but Phase II should show less exploration than Phase I.

To put our expectations in a more precise and statistically verifiable form, the following predictions follow from our theory and can be tested by our experiment:

Prediction I. In Phase III, Experimental Group, the mean exploration of the odd (novel) stimuli will be significantly greater than the mean exploration of the similar (familiar) stimuli.

Prediction II. In Phase I, Control Group, the mean exploration of the odd stimuli will *not* be significantly greater than the mean exploration of the similar stimuli.

Prediction III. In Phase III, Experimental Group, the mean exploration of the similar stimuli will be significantly *less* than the mean exploration of the similar stimuli in Phase I, Control Group.

Prediction IV. In Phase II, both groups, the mean exploration of all stimuli will be significantly *less* than the mean exploration of all stimuli in Phase I, both groups.

Results

The behaviour of all rats in the experimental box followed more or less the same pattern. When first put into the box, each animal remained for some seconds in a state of tense alertness. It then advanced cautiously and, on reaching the stimuli, sniffed at them, but exploration of rings or cubes never took up more than a small fraction (maximum = 39 sec. Rat A, Phase I) of the five-minute period. After this brief and cursory investigation it invariably found some position in a corner or by a wall of the box in which to settle. It generally remained there quiescent till taken out of the box, but some animals roused themselves after a few minutes and took a second walk round, giving a few additional sniffs at the stimuli as they passed. One or two even picked up one of the rings in their mouths and played with them.

The numerical results, i.e. the seconds of exploration devoted to each stimulus, are shown in Tables 1 and 2:

Table 1. *Experimental Group*

Key: 0 = ring; X = cube (odd stimulus in italics). *S* = Stimuli used, from left to right; *E* = time of exploration of each stimulus to nearest second.)

		Cube Sub-Group			Ring Sub-Group		
	Subject	A	B	C	D	E	F
Phase I	*S*	X–X–X	X–X–X	X–X–X	0–0–0	0–0–0	0–0–0
	E	13–13–13	0–1–7	5–4–1	2–3–1	1–3–4	2–8–2
Phase II	*S*	X–X–X	X–X–X	X–X–X	0–0–0	0–0–0	0–0–0
	E	1–2–0	0–0–1	0–0–0	0–1–5	0–0–0	0–0–0
Phase III	*S*	*0*–X–X	X–*0*–X	X–X–*0*	*X*–0–0	0–*X*–0	0–0–*X*
	E	*8*–7–2	0–*4*–1	0–2–*1*	*4*–1–3	1–*3*–1	1–1–*2*

We can now see whether our predictions are fulfilled.

Prediction I (Phase III, Experimental Group). For each rat we take the quantity of exploration of the odd (novel) stimulus and subtract from it the mean of the quantities of exploration of the two similar (familiar) stimuli. In other words, if O = exploration of odd stimulus, and $S_1 + S_2$ = exploration of two similar stimuli, we take the quantity

$O-\frac{S_1+S_2}{2}$. If the mean of this quantity for the six rats is positive and significant, the prediction is confirmed.

Results. Mean $O-\frac{S_1+S_2}{2}=2{\cdot}00$; $t=3{\cdot}55$; $\nu=5$; $p=0{\cdot}017$.

The result is positive and significant at the 0·02 level and the prediction is fulfilled.

Prediction II (Phase I, Control Group). Again we take the mean of $O-\frac{S_1+S_2}{2}$ for six rats. If this mean is positive and significant, the prediction is falsified.

Results. Mean $O-\frac{S_1+S_2}{2}=-0{\cdot}17$.

The result is therefore not positive and significant and the prediction is fulfilled.

(The difference between the means of $O-\frac{S_1+S_2}{2}$ for Phase III, Experimental Group and Phase I, Control Group is not significant ($t=1{\cdot}48$ $p=0{\cdot}17$).)

Table 2. *Control Group*

(Key as above.)

		Controls for Cube Sub-Group			Controls for Ring Sub-Group		
	Subject	A′	B′	C′	D′	E′	F′
Phase I	*S*	*0*–X–X	X–*0*–X	X–X–*0*	*X*–0–0	0–*X*–0	0–0–*X*
	E	*2*–0–0	7–*12*–9	4–7–*7*	*2*–3–2	7–*2*–6	3–8–*2*
Phase II	*S*	*0*–X–X	X–*0*–X	X–X–*0*	*X*–0–0	0–*X*–0	0–0–*X*
	E	*0*–0–0	3–*9*–8	3–3–*3*	*3*–0–1	1–*5*–2	2–3–*2*

Prediction III (Phase III, Experimental Group, Phase I, Control Group). We take the mean exploration-time for the two similar stimuli of the six rats in the Experimental Group and the same for the six rats of the Control Group. If the latter is significantly greater than the former, the prediction is fulfilled.

Results. Mean exploration of similar stimuli (Phase III, Experimental Group), 3·33. Mean exploration of similar stimuli (Phase I, Control Group), 9·33; $t=2{\cdot}27$; $\nu=10$; $p=0{\cdot}046$.

The result is therefore significant at the 0·05 level and the prediction is fulfilled.

Prediction IV (Phase I and Phase II, both groups). We take the mean decrease in exploration of all stimuli from Phase I to Phase II for all twelve rats. If this mean is positive and significant, the prediction is fulfilled.

Results. Mean decrease = 9·00. $t=3{\cdot}40$; $\nu=11$; $p=0{\cdot}006$.

The result is therefore significant at the 0·01 level and the prediction is confirmed.

Discussion. All four predictions are thus fulfilled by the experiment and the theory is therefore confirmed, in the sense of 'rendered more probable'.

Many problems still remain to be solved. Apart from the reservations mentioned in connexion with the postulates, we require to know more about other forms of motivation to explore, e.g. the instrumental exploration and surprise-exploration described above, and their relations to the form of motivation, due to novelty, which we have called 'curiosity'. We need to link the exploratory activity that is found in the lower animals, such as the rat, with human exploratory activity. In particular, we must investigate the symbolic processes that enable human curiosity to be satiated by an 'explanation' rather than by sensory contact with the object of curiosity.

But it is hoped that the present study will have helped to indicate some of the variables at work and thus some of the questions further research may profitably pursue.

VI. Summary

The scope and importance of novelty, an unjustly neglected topic in both human and animal psychology, is discussed. Some theories of curiosity are reviewed and problems arising out of them are considered.

A new theory to deal with exploratory behaviour in response to novelty is then presented. It consists of two postulates:

Postulate I. When a novel stimulus affects an organism's receptors, there will occur a drive-stimulus-producing response ($R \rightsquigarrow S_D$) (*which we shall call 'curiosity'*).

Postulate II. As a curiosity-arousing stimulus continues to affect an organism's receptors, curiosity will diminish.

If these postulates are used in conjunction with Hull's behaviour theory, three corollaries follow which describe the functioning of exploratory activity.

An experiment with rats is then reported. The experiment demonstrated (*a*) that rats will spend more time exploring a novel stimulus than stimuli which they have previously been able to explore; and (*b*) that they will spend less time exploring stimuli the second time they encounter them. In particular, four predictions follow from our postulates and corollaries. All four are confirmed by the results of the experiment.

The writer wishes to express thanks to the following: the Executive Committee of the Carnegie Trust for making a grant to cover the expenses of this investigation; Mr H. H. Ferguson for helping and supporting the project in various ways; Mr M. J. Sweeney for invaluable technical assistance; Prof. R. Walmsley for allowing the subjects to be accommodated in the animal-house belonging to St Andrews University, Anatomy Department.

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(*Manuscript received* 24 *February* 1950)

CRITICAL NOTICE

By DAVID J. STEPHENS, *University College of Swansea,*
with a Reply by Christina A. Smith *and a Note by* D. N. Lawley

Mental Testing of Hebridean Children in Gaelic and English. By Christina A. Smith. With a Statistical Analysis by D. N. Lawley. Publications of the Scottish Council for Research in Education, No. xxvii. London: University of London Press. 1948. Pp. 42. 1*s.* net.

This account of the mental testing of Hebridean children is an important contribution to our knowledge of the educational problems of an isolated rural and bilingual community. Miss Smith has given us a clear picture of educational conditions in the Hebrides and of the difficulties of the testing programme, and on the basis of this investigation has made many shrewd observations on the mental processes of bilingual children. We would, however, have been more grateful to Miss Smith if she had presented us with a less confused account of the result of the inquiry. In her presentation of the findings of the research she has been influenced by opinions based on impression and 'superficial observations', despite the fact that the results of an excellent statistical analysis by Dr Lawley were available to her. This is doubly unfortunate as many readers, ill-equipped to follow the statistical analysis, will assume that these opinions are supported by the results of that analysis.

The main inquiry in 1945 had been preceded by two investigations: the first was an investigation using Cattell's Non-Verbal Intelligence Test, Scale 1, Form A in English and Form B in Gaelic, which was administered to a random sample of unstated number of the bilingual children in Lewis; the second consisted of individual tests given to ten bilingual children using the Terman-Merrill Revision of the Binet Scale, Form L in English and Form M in a Gaelic translation. In Cattell's Test, Form B has been standardized to follow Form A; this makes allowances for any practice effect which may result from the administration of the first test. In the preliminary investigation, a statistically significant difference was found in the sample tested between scores obtained on Form A in English and on Form B in Gaelic, a difference over and above the allowance which Cattell would attribute to practice effect. It was also found that the mean I.Q. of this bilingual group was depressed by approximately 15 points.

The main investigation was designed to 'investigate the variances in scores between first and second test in all possible orders of test forms, in conjunction with all possible language sequences'. The 415 bilingual children who were tested were divided into three age groups; they were either age eight, ten or twelve years old at the date of testing. Each age group was also divided into eight to correspond with the two possible test orders and the four possible language sequences. The detailed results of the investigation are published in the Appendix.

What were the statistical findings of this research? Dr Lawley, in his analysis of the raw score data, adopts a technique suggested by Fisher in his *Design of Experiments* and *Statistical Methods for Research Workers*. The analysis of variance showed that:

(1) 'apparent differences between the three age groups were not significant' (p. 34);
(2) 'neither differences between form order nor
(3) differences between language combinations attain significance' (p. 36);
(4) 'there is no obvious dependence upon practice effect of level of performance' (p. 36).

Dr Lawley infers from these statistical findings that the average score increase 'is substantially the same whether Form A or Form B is given first, i.e. that the two forms can be assumed of equal difficulty. Secondly, we are justified in concluding that the practice effect is the same whatever the language in which the tests are given. The results of the experiment would seem to indicate that the children were equally good at comprehending English and Gaelic.'

The footnote by Prof. Godfrey Thomson points out that it must not be inferred that there is no all-round handicap from the introduction of the second language.

The inference drawn by Dr Lawley that the children 'were equally good at comprehending English and Gaelic' is not logically necessary. The analysis shows that the practice effect is the same whatever the language-sequence in giving the tests. Before arriving at Dr Lawley's conclusion, we should examine the

alternative conclusion that the practice effect is independent of the language used in the second test. If it were possible to devise non-verbal instructions for the second test, would similar practice effects be observed? Do children recognize the similarities of the test items of Form A and Form B; and ignore instructions in the second test to concentrate on the pictures?

Inspection of the two tests reveals that, with the exception of the 'Instructions', linguistic instructions are not essential in the second. It would seem, therefore, that before a decision can be made on the question of whether the practice effect is independent of the language used in the second test or is due to equal facility in comprehending English and Gaelic, a further inquiry is needed to determine the precise practice effects in 'Instructions' and the remainder of test.

The findings of Dr Lawley's analysis are not specifically quoted in Miss Smith's report of the results. Instead we are presented with some 'general impressions derived from the testing. That these impressions often contradict the results of the statistical analysis in no way embarrasses Miss Smith in drawing the conclusion that 'to sum up the general findings of the series of tests administered, bilingual children seem to respond more adequately when tested in their native tongue—Gaelic'. It is true that this statement follows a detailed description of the individual tests administered to ten children, from which small number of cases 'it would be useless to generalize'. We note, however, that, despite Miss Smith's good intentions, she has generalized on these ten results or has completely falsified the findings of Dr Lawley's analysis which give no support to there being any difference in the children's comprehension of English and Gaelic.

We have questioned the logical necessity of Dr Lawley's conclusion that, if the practice effect is independent of the language sequence, the children are equally good at comprehending English and Gaelic. But

Table 1. *Mean scores in initial test*

Age group (years)	Form A		Form B	
	Gaelic	English	Gaelic	English
8	14·1	16·1	15·5	12·0
10	31·0	38·8	31·1	31·6
12	46·6	54·4	50·3	56·9

Table 2. *Values of 't'*

Form A

Age 8	1·0	(Not significant)	
Age 10	2·4	Significant	Probability of only 0·02 of a greater numerical value of '*t*' occurring
Age 12	2·4	Significant	

Form B

Age 8	2·0	(In favour of Gaelic)	Probability of 0·05 of a greater numerical value of '*t*' occurring
Age 10	—	Not significant	
Age 12	2·0	—	

we do not question the actual result of the analysis. Miss Smith, however, ignores this statistical conclusion. On p. 28 she tells us that 'the writer formed the opinion that the increases are greater in some degree when the language used in the first test is Gaelic. There is one fairly obvious interpretation of this, which is that after hearing the original instructions in Gaelic, the child is thoroughly cognizant of the test procedure, so increasing his efficiency and speed during the second trial.' This opinion is stated to be based on 'purely superficial observation', which is thus given as much weight as the statistical analysis. We are surprised also that an equally obvious interpretation of this superficial observation should not have occurred to Miss Smith. Poor comprehension of the original instructions in Gaelic would depress the scores in the first test so that, as the children 'tumbled' to the requirements of the test, greater practice effects would be possible. Of course this explanation also assumes that the analysis is subject to a Type II error in finding the practice effect independent of the language-sequence and should for that reason be rejected: but it does focus attention on the value of the initial mean scores in determining whether the children are equally good at comprehending English and Gaelic. The initial mean scores for the twelve groups show better responses to English in five out of the six pairs. This is brought out in Table 1 above.

It is natural to ask whether these differences in mean scores are significant. Values of '*t*' are given in Table 2, except for the obvious case of non-significance, i.e. for age 10, Form B.

We are thus led to the conclusion that the children of age 12 respond better to test instructions given in English than in Gaelic. May we infer that, at the age of 12, Hebridean children understand English better than Gaelic? This would seem to be the obvious explanation, yet we hesitate to adopt it unreservedly

without the positive support which might have been provided by the answer patterns of the first and second tests.

The level of performance in the test seemed to be determined by the linguistic instructions in the first test, and in the second by the language used in the first together with a practice effect which is independent of the language used in the second.

The points raised in this examination of the results require a further inquiry before a valid theory can be proposed. The answer patterns for the first and second tests will confirm or deny the suggestion that the children tend to ignore the linguistic instructions in the second test. If this suggestion be true, we should find that the language-sequence has an influence on 'Instructions'; for in this the comprehension of language is essential and the analysis of mean scores has shown that there is a significant difference in comprehension among the older children.

It is gratifying to find that the raw material of an important inquiry of this kind has been made available for further study and we thank Miss Smith and Dr Lawley for their important contribution on the testing of bilingual children.

REPLY

By Christina A. Smith

In 'A Critical Consideration of *Mental Testing of Hebridean Children in Gaelic and English*' Mr Stephens appears to be guilty of misrepresentation. It should be clearly stated that the first section of the publication is a descriptive account of the experiment undertaken in Lewis, while the second section comprises the statistical analysis *subsequently* performed by Dr Lawley, the two sections being to that extent discrete.

Mr Stephens complains of the 'confused account of the results of the inquiry'. The observations, to which he refers, are, as emphasized in the context, in the nature of prediction on a subjective basis, pending the preparation of a rigorous objective statement. (That the prediction was later disproved is not without interest in any impartial assessment of an experiment, where so many factors are operative.) The statistical analysis, then, was not, as stated by Mr Stephens, available when the first section of the report was presented. For who, having even an elementary grasp of scientific method, would be disposed to re-interpret, or misinterpret, objective findings with the abandon he alleges? While points raised in the substance of the first section may be of passing interest, it is clearly to the statistical analysis that the reader looks for a statement of results. That statement is so lucidly presented that further comment and elaboration elsewhere in the report would obviously have been superfluous.

In Mr Stephens's detailed study, which raises several issues, his alternative interpretation of marked practice effect is of interest. Further scrutiny of the possible independence of language of the observed practice effect might prove rewarding, but 'Instructions', a short sub-test of about a dozen items, might not give effective discrimination throughout the age range, because of restricted 'footroom' in the case of the youngest group and restricted 'headroom' in the case of the oldest. It might be advisable to use an alternative means of measurement, specifically devised for the purpose in view, the probability being that recognition of similarity of test and retest—independently of language—is a function of mental maturation. Such an investigation could perhaps lead to more precise statement of the various factors contributing to the significantly high practice effects recorded.

The statement in the report that 'increases are greater in some degree when the language used in the first test is Gaelic' is described by Mr Stephens as falsification, whereas in actual fact it was a subjective estimate. Moreover, the statement is quantitatively true, the exact 'degree', however, being such that by the final criterion of statistical significance it is negligible. It is in dealing with this issue that Mr Stephens pauses to insinuate that 'poor comprehension of the original instructions in Gaelic would depress the scores in the first test...'. Statistical faith should not blind us to reality, surely. Is comprehension of one's *native* language likely to be so poor that, in general, subsequent 'tumbling' operates in reverse, so to speak, effecting large increases in second score?

Theoretically the tabulation of mean scores in the initial test is a sound basis of comparison, but are the inferences drawn by Mr Stephens strictly justifiable? The question of numerical inequality between sub-groups should be raised in this connexion. The procedure of analysis of variance was adopted precisely to avoid possible fallacy in calculating means from small numbers. This table, showing the number of cases yielding mean scores as in Table 1, emphasizes such inequality:

Age group (years)	Form A		Form B	
	Gaelic	English	Gaelic	English
8	29	39	27	60
10	25	41	32	31
12	35	37	39	20

Although, for example, a sub-group of 25 receives treatment of mean score on equivalent terms with a sub-group of 41, we may not be strictly justified in assuming that normal sampling has been achieved. Mean scores in such a case might not be truly representative of the population under consideration. But if the test forms be accepted as equal in difficulty, a composite table could indicate the general trend of comparative scores in first tests. Such a table, increasing the numbers in each group and thereby reducing the chance of a significantly skew distribution, yields the following mean scores:

Age group (years)	First test in Gaelic	First test in English
8	14·7 (56 cases)	13·6 (99 cases)
10	31·1 (57 cases)	36·0 (72 cases)
12	48·5 (74 cases)	55·3 (57 cases)

(It is unfortunate, as far as refinements of statistical procedure are concerned, that circumstances in the experimental field prevented equality between groups, and it is important to note that the distribution of cases as grouped above may still deviate from the normal within the sub-groups.)

Acceptance of the validity of the above mean scores seems to corroborate the inference drawn by Mr Stephens, 'that Hebridean children at age 12 understand English better than Gaelic...'. The correct form of the inference is more probably that Hebridean children of age 12 are more accustomed to school-room activity, such as a test, conducted through the medium of English, and so make a better response to test instructions in this language. Greater facility in English, in the case of the 12-year-old group, is apparently associated with the specific environment of school. Situations outside school and in the home would tend to be more adequately dealt with through the medium of Gaelic.

Mr Stephens's analysis redirects attention to the familiar problems involved in bilingualism. That fluency in English increases, with each year of schooling, towards final mastery is beyond dispute. For practical purposes we should rather consider the possible nature, and degree, of the interference of one language with the other in the course of education.

A NOTE

By D. N. Lawley

I agree with Mr Stephens that it is certainly not a logically necessary conclusion that the children 'were equally good at comprehending English and Gaelic', only a possible explanation of the data.

My second point is that, in my opinion, very little can be drawn from comparisons made in his Table 1, since the groups of children were not selected at random, a fact which in itself would be sufficient to account for the significant values of t obtained.

CORRIGENDUM

'Variations in the performance of some Aestheto-kinetic Tests.' By J. R. Manning.

The equation given in paragraph IV (*b*) on page 223 of Vol. xl of this *Journal* (June 1950) should *read*:

$$F = 267{\cdot}6 - 1{\cdot}817$$

PUBLICATIONS RECENTLY RECEIVED

The Organization of Behaviour. By D. O. Hebb. London: Chapman and Hall Ltd. 1949. Pp. xix+335. 32*s*. net.

Huxley once remarked that '...those who refuse to go beyond fact rarely get as far as fact....' In this very stimulating book Hebb has recognized the importance of system-making and has presented a grand scheme for 'The Organization of Behaviour'. He has undertaken the ambitious task of presenting a conceptual system '...which relates the individual nerve cell to psychological phenomena....' This does not mean that 'facts' are seen to play a secondary role, for the proposed theoretical model is based upon some new and some familiar empirical observations culled from research in several sciences and presented with emphasis which should prove novel to most readers. The model is applied initially to the analysis of the development of simple visual perceptions. It is then elaborated to account for the actions of attention, motivation, emotional disturbances, learning capacity, and 'intelligence' in 'the cerebral control of behaviour'. An effort is made throughout the system to arrive at conceptions which are as faithful to present physiological fact as possible.

The general schema proposes two important intervening variables as basic to the organization of behaviour. The first concerns the direct effect of environmental changes on activity in the central nervous system. Repeated stimulation of specific receptors is assumed to lead to the establishment of a 'cell-assembly' in the association areas of the brain. This assembly is capable of continued activity for brief periods following the cessation of sensory stimulation, a property of reverberating circuits in the central nervous system. This after-effect provides opportunity for growth changes—structural or metabolic—to take place which make the synapse more readily traversed, and which are basic to the permanence of behaviour changes. So far, the theory is a form of connexionism and so, in fact, it remains, although the connexions are not directly between stimulus and response, but rather '...serve to establish autonomous central activities, which then are the basis of further learning'.

The cell-assembly has other characteristics which are important in the control of behaviour. Anatomically it is assumed to be '...a diffuse structure comprising cells in the cortex and diencephalon (and also, perhaps, in the basal ganglia of the cerebrum)...' This diffuseness provides for functionally similar, alternate pathways, some of which may be non-functioning (e.g., in the case of brain damage) without destroying the dynamics of the total assembly. The importance of very delicate time-relations in the neutral firing of the cell-assembly is emphasized and it is assumed that any disturbance of timing—whether due to metabolic changes (e.g. in hunger) or to conflicting sensory stimulation—disorganizes behaviour. Such effects when temporary lead to 'emotional disturbances', when chronic, to neurosis or psychosis.

The second major intervening variable is called the 'phase sequence.' Phase sequences result from the successive action of a series of cell-assemblies. Association of this kind produces interfacilitation between individual assemblies and initiates growth changes resulting in an essentially new, superordinate system in central nervous structures. In such a system the activation of one set of assemblies results in the arousal of the others. This means that assemblies may be aroused indirectly by the action of other assemblies, which in turn, were activated by direct stimulation. Activity so aroused is referred to as 'conceptual' or 'ideational' activity. It is this type of series with its motor elements that is referred to as a 'phase sequence'.

The various phenomena of behaviour are analysed in terms of this general schema. Motivation is accounted for in terms of the direction or content of an existing phase sequence and in terms of its persistence and stability. Insightful behaviour results from '...a "chance" combination of facilitations from different phase sequences....' Emotional disturbance is basically '...a disruption of neural activity in the cerebrum...' which may cause modifications of existing superordinate systems (thought processes). When such disturbances are chronic or modifications lasting, the organism develops the symptoms of neurosis or psychosis. 'Intelligence' has two supplementary components: an 'innate potential' defined in terms of brain structure and neural metabolism, and a developmental component based upon the formation of superordinate systems during an individual's ontogeny. Hebb has considerable success in fitting into his schema these and other behavioural phenomena which have perplexed behaviour theorists for a long time.

Regardless of the alterations which new information undoubtedly will require, the schema does serve to organize present knowledge and to suggest interesting lines for future research. A short review cannot do justice to the scope and detail of this book. Those interested in behaviour theory will find it stimulating to examine *The Organization of Behaviour* much more carefully.

R. W. Russell

Annual Review of Psychology. Ed. Calvin P. Stone and Donald W. Taylor. Stanford, California, U.S.A.: Annual Reviews Inc. 1950. Vol. 1. Pp. x+332. $6.00 net.

Many psychologists have been indebted to that admirable publication *Annual Review of Physiology*, which for a number of years has included excellent surveys of work lying on the border of the two subjects. The enterprising publishers have this year decided to stand Psychology on its own feet, and the resulting volume bears at least an external resemblance to its elder brother. Physiology and psychology alike are embarrassed by a rapidly expanding output and increasing specialization, and it might well be thought that what the older publication accomplished for the former, the newer should achieve for the latter. Surveys of a limited number of leading topics thoughtfully selected, and provided with economically conceived bibliographies, should at least carve a trail through the jungle of the journals for the harassed specialist with little time for reading outside his own subject. Such, in part at any rate, is the intention of the publishers and editors, and in part it is fulfilled. But not even the admirable team of contributors here at work can infuse into psychology at large that agreed basis of language and method which allows physiology to profit from this kind of exposition. Some of the writers in this volume have let the demands of succinctness force them into the use of a jargon too parochial for general comprehension. In some chapters, too, evaluation is a trifle forced and perfunctory. It may be feared that parts of the book may do more to fortify the verbal façade, than to deepen the understanding, of the reader. Certainly the pious hope expressed in the preface that the work may guide nurses and other professional folk in their perusal of psychological literature must be regarded as verging on the unrealistic.

Editors and contributors alike are certainly aware of these and other difficulties in what is the initial stage of an important venture. It is for the reader to be gratefully appreciative of their courage and enterprise. While there is not, and could not be, any claim to completeness of survey, the eighteen chapters cover a very wide range of topics, from vision to statistical technique, and from learning to counselling methods. Space forbids detailed mention, but Hebb's chapter on Animal and Physiological Psychology comes nearest, perhaps, to suggesting a model for future contributors. It is simple, informative, readable, and something better than comprehensive in intention.

This book, with its successors, will be of great usefulness, and is of the kind that should be in every library.

R. C. OLDFIELD

The British Journal of Sociology. Vol. 1, no. 1. London: Routledge and Kegan Paul. March 1950. Pp. iv+92. Single Copy 10*s.* net; annual subscription 30*s.* net.

On the publication in March of the first number we welcome the *British Journal of Sociology* under the editorship of Prof. Ginsberg, Prof. Glass and Prof. Marshall. The editorial board is appropriately located at the London School of Economics, the headquarters of British Sociology. It really looks as though we shall at last be adequately represented among the sociological journals of the world.

This number contains the first instalment of an article by Prof. Aron on 'Social Structure and the Ruling Class'. Prof. Aron provides an interesting analysis of the notions of class and *élite*, and then proceeds to describe the lop-sided structure of French society in the years immediately after the war, when there was a disproportion of what Colin Clark calls 'tertiary activities'—public services, commerce and administration. Too many 'parasites' upon the emaciated body politic upset the relations between political and economic power.

Prof. Glass's Inaugural Lecture follows. In it he makes a well-documented plea for social research in the framing and implementation of policy. The next article is a 'first report of work undertaken as the result of a grant received by the London School of Economics from the Trustees of the Nuffield Foundation'. The research in question is concerned with class structure and class mobility, and this interim report deals with the preliminary problem of occupational prestige. Happily John Hall and Caradog Jones, the authors, find that there is general agreement about the prestige-value of occupations, though some readers will be surprised to find that barmen tend to be rated lower than railway porters.

Dr Eysenck supplies a valuable and characteristically incisive criticism of the methods employed by Centers in his book on *The Psychology of Social Classes*. Finally, we come to what many may find the most interesting article of all. Mr Asa Briggs in a paper called: 'Social Structure and Politics in Birmingham and Lyons' compares the movements in these two cities between the years 1825 and 1848. Such an enterprise might sound quite fantastic, but it turns out that these two cities have certain significant similarities and equally significant differences, and their history in these revolutionary years is extremely illuminating. Mr Briggs provides a type instance of the value of the 'comparative method' in sociological analysis.

It will be seen that the first number of the new journal has rather concentrated on the problems of class

and there is much to be said for a dominating theme. We can rest assured, however, that the editors do not intend to limit the range of discussion to this particular aspect of social structure and that they will preserve the high standard they have already established in every field of inquiry which they wish to bring to the notice of students of sociology. W. J. H. SPROTT

Studies in Human Behaviour. By MERLE LAWRENCE. Princeton University Press (London: Geoffrey Cumberlege). 1949. Pp. x + 200. 28*s.* net.

This book is described on the title page as 'A Laboratory Manual in General Psychology organized to stress the basic principles of individual and group behaviour with emphasis on perception', and contains twenty-four experiments designed to meet the needs of an introductory course at Princeton. These experiments are arranged in a definite sequence under the headings: Sensory Experience, The Nature of Experience, Social Factors and Perception, The Process of Learning, and Individual Differences, so that the student progresses from a study of the individual's perception and response to external stimuli, to the effect of past experience on later behaviour, and then is introduced to tests of Mechanical Aptitude, Intelligence, and assessment of Personality. Instructions are given to the student, not only on general laboratory procedure, but also precisely how to conduct each experiment, and appropriate spaces are left for him in which to fill his own results. A certain amount of explanatory theory is also included, and there are clear photographs of the apparatus used.

While it is interesting and valuable for purposes of comparison to have such a clear picture of a planned laboratory course which has been tried and found satisfactory, it is unlikely to find use in this country as a ready-made course, partly because the apparatus and equipment may not be available, and because those who are responsible for such courses will prefer to arrange them to suit the particular conditions in their own laboratories. The high price of the book owing to the devaluation of the pound places it at a serious disadvantage. S. B. N. SHIMMIN

Human Ability. By C. SPEARMAN and LL. WYNN JONES. London: Macmillan. 1950. Pp. viii + 202. 16*s.* net.

When the late Prof. Spearman published *The Abilities of Man* in 1927, the endeavour to establish the nature of human abilities by the method of factorial analysis had already made some progress. *The Abilities of Man* was a noteworthy contribution in this field, and though this latest volume, subtitled 'A continuation of the Abilities of Man' is again a review of the evidence from factorial analysis during the past twenty years, it cannot be said to add much to the principles already set forth in the earlier volume. The first part of the book consists of a restatement of the monarchic doctrine of '*g*' which the authors believe has not been invalidated by later evidence. The last portion of the book is devoted to a brief restatement of the views expressed in *The Abilities of Man* on Speed, Retentivity, Oscillation and Fatigue. It is the central part of the book which is most interesting. Here is examined the evidence for the 'group' factors, verbal, mechanical, number and the like. The test applied is a strict logical one, the method of agreement and difference, and the authors find the evidence not wholly adequate to establish clearly such factors both logically and psychologically. Those who perform various arithmetical operations on a correlation matrix, and too readily label the resulting factors as space, reasoning and such like, could read these chapters with profit.

Prof. Spearman did not live to see the book completed; the final preparations for publication were made by his friend and collaborator, Dr Wynn Jones. A certain unevenness in presentation has resulted, the style being in places of an almost cryptic conciseness. But an assessment of later progress by two of the pioneers in the field of factorial analysis cannot fail to be of interest; and the application to these investigations into human ability of the basic principles of logic and psychology by a mind so clear and critical as Prof. Spearman's cannot fail to be of value. JAMES MAXWELL

Psychological Statistics. By QUINN MCNEMAR. London: Chapman and Hall. 1949. Pp. vii + 364. 27*s.* net.

This work will certainly prove valuable as a general reference book on standard procedures, and may well be found suitable as a text-book for a two-year college course. I believe it has already been adopted for this purpose in some universities; but the standards of general mathematical knowledge it pre-supposes may be found a little too exacting and the use of algebraic notation a little too lavish to commend it for use

in courses for students who are taking psychology in an Arts degree. And on the other hand, students with a sound mathematical background who are willing to devote a good deal of attention to the applications of statistics to psychology may well regret that methods of factor analysis are not described and discussed.

No doubt Prof. McNemar has felt it wisest to omit this subject because of the controversies which still surround it. His treatment follows traditional lines and there are no startling innovations. If anything he has erred on the side of conservatism. For instance, he still uses sigma to denote the standard deviation in a sample, although it is becoming increasingly customary to use this and all Greek lettering only in reference to population parameters. Small-sample theory is treated as an addendum to large-sample theory and the notion of degrees of freedom is not introduced until after the middle of the book, although many advantages are to be gained by introducing this subject early and treating large-sample theory as a limiting case of principles applying to all sizes of samples generally.

These are minor criticisms of a work which is sound and scholarly throughout and will certainly find many uses. PATRICK SLATER

Introduction aux Méthodes Statistiques en Psychologie Appliquée. By J. M. Faverge. Paris: Presses Universitaires de France. 1950. Pp. xvi + 164. 480 fr. net.

In the preface to this small, paper-backed book, Dr A. Ombredane remarks that the method of presentation will cause a student of psychology, who is afraid of statistical work, to exclaim, 'But it is much easier than I thought!' There is a fair degree of truth in this comment, for the author uses only simple arithmetic and algebra in his exposition, but there is also a great danger that the inexperienced student will think that the statistical approach to psychological experimentation is too simple. As is perhaps inevitable in an elementary treatise of this sort, many of the methods described are for rule-of-thumb application and the assumptions underlying them and the cares needed for interpretation are not always sufficiently emphasized. The author covers a wide range of methods, from the stock ways of calculating the arithmetic mean and standard deviation to a simple case of the analysis of variance and the analysis of co-variance, and even includes an example of the use of Aitken's method of pivotal condensation in the calculation of multiple correlation coefficients. The applications of the methods to psychological work are confined very largely to the use of tests, and the great difficulties encountered by the applied psychologist in searching for adequate criteria against which to validate his tests are rather overlooked. Also, the advice to normalize rating scales can be very dangerous to a beginner. On the whole, the book is a useful introduction to statistical methods for a student working under experienced supervision. E.G.C.

The Psychology of Social Classes. A Study of Class Consciousness. By Richard Centers. Princeton University Press. (London: Geoffrey Cumberlege). 1949. Pp. xii + 244. $3.50 or 22*s.* 6*d.*

During the last decade the social sciences have become increasingly concerned with the problems of social class. Perhaps this is not unrelated to the quickening pace of social change brought about by the war.

Dr Centers begins and ends his book with reflections upon the threatening rumblings of incipient class-conflict in America. Although it is easy to guess his political orientation, this does not seem to have influenced his research. All the same, one might wonder how he would use greater knowledge of social class which, as he puts it 'would be a tremendously valuable asset from the standpoint of psychological manipulation and control of class attitudes and behaviour.'

In the introductory part existing theories of social class are examined. These are divided into two broad groups, objective and subjective. In the first type classes are defined by objective criteria, income or occupation for instance. Centers holds that such a view confuses classes with strata. He himself comes down on the side of the subjectivists who regard classes as primarily psychological groupings characterized by feelings of belongingness. These feelings, together with a cluster of specific attitudes arise, he maintains, as a result of the individual's position in relation to the economic processes of society. Such, very roughly, is the 'interest-group' theory of social class he adopts.

The method used to give more precise content to this theory was a public-opinion type of survey with a quota sample of 1100 men. The informants were asked whether they felt they belonged to the upper, middle, working or lower class. Other questions attempted to tap the Conservative-Radical dimension of attitudes, voting behaviour, union membership and many others.

The results, presented in a massive array of tables and graphs, turn out to contain little that is new. Thus we learn that there are significant differences in Conservatism-Radicalism between various occupa-

tional strata. A similar relation is found between the latter and political behaviour. People in the lower economic groups show greater union membership and tend to approve more of unionism. The marked correspondence between occupational stratification and class-affiliation is also not very surprising. However, it will be of interest to readers familiar with the work of the Nuffield Research Unit at the London School of Economics that there is far-reaching agreement among the various classes concerning the social grading of occupations, a conclusion parallel to that reached in this country.

Centers also attempted to get at the determinants of class-consciousness. He did this by means of elaborate analyses and partial correlation techniques, but the outcome is not convincing; the kind of material he collected is hardly suitable for the purpose of causal analysis.

It would be unfair to complain that Centers has not made any startling discoveries. A great deal of work in the social sciences consists of the painstaking checking and quantitative formulation of common-sense observations; and occasionally common sense is then seen to have gone far astray. But there appear to be some fundamental weaknesses in Centers's approach. First, the classes he describes are in a very real sense his own creations. He presented his informants with four categories in which to place themselves: upper, middle, working and lower class; had he used Lloyd Warner's classification his subjects would no doubt have been equally obliging, with the picture obtained being a rather different one. In other words, the study was set up in such a way that something like the pattern he found *had* to emerge.

Secondly, his concept of social class is one-sided in that it neglects some important factors implied by common usage of laymen and social scientists alike. His concept is focused on the social atom and built up on the basis of its responses taken in isolation. A brief example will make this clearer:

17% of large business men described themselves as 'working-class'.

18% of unskilled labourers described themselves as 'middle-class'.

According to Centers what a man feels himself to be, in terms of class, he is: would his neighbours share this view? Is it reasonable to say that the large business men really are working-class? This objection cannot be dismissed, as Centers tries to do, by saying that this is something quite different: social rank, prestige, or status. What a person feels himself to be cannot, in the social setting, be divorced from the way his fellows regard him. The inmate of an institution is a Napoleon only in his fantasy world; the two semi-skilled workers who said they were upper-class indulged perhaps in some mild leg-pulling.

This leads to the final point: are cafeteria-type questions and simple attitude scales suitable instruments for eliciting the subtle nuances of class-feeling with their frequent undertone of anxiety? The answer of most of those who have worked in this field is likely to be in the negative. Centers used a large net with coarse meshes in his endeavour to catch very small fish; as a result he pulled out only the large ones, already familiar to most of us.

G. JAHODA

The Pre-Election Polls of 1948. By Frederick Mosteller, Herbert Hyman, Phillip J. McCarthy, Eli S. Marks and David B. Truman. New York: Social Science Research Council, Bulletin 60. 1949. Pp. xx+396. $2.50 paper, $3.00 cloth.

This book was written and the research reported in it initiated because of the failure of the various polling agencies to forecast the outcome of the presidential election in 1948 correctly. The committee organized this work with the full co-operation of the major polling organizations who opened their files and made their staff available for interrogation and discussion.

The speed with which the Social Research Council acted in this matter and the competent way in which they carried out their work, enable us to have a much clearer idea why the poll went wrong upon that occasion.

The book throughout maintains a high level of scientific competence and within the limit set by the material available it is an excellent job indeed. It is packed with facts; indeed it is so full of tables, percentages and graphs that no brief summary is possible.

Some of the points investigated and stressed in this study have already received a good deal of discussion in the pages of the *International Journal of Opinion and Attitude Research*, such as, for instance, the last-minute swing to Truman, the problem of the allocation of undecided voters to one or the other party, and the question of quota versus area sampling. One point emerges most clearly from this study. The actual percentage error of forecast was not very much higher than the average error made by the polls over a period of years; what was at fault was not so much the method of arriving at the forecast, but rather the all-or-none character of the way in which this forecast was presented to the public. This leads to the curious position that if the polling agencies find that the most likely poll for candidate A will be 65%, and 35% for candidate B, and if in actual fact candidate A polls 55% and candidate B 45%, the error in forecasting will

be 10%, but the right candidate will be chosen and the public will regard this as a success for the polls. However, if candidate *A* is forecast to win 55% of the votes and candidate *B* 45%, and in actual fact candidate *A* has 49% and candidate *B* 51%, the average error will be only 6%, but as the wrong person will have been predicted the public will be righteously indignant and demand the head of the pollsters.

This could be avoided, of course, by including a warning regarding sampling errors and other technical points in the actual forecast, but as the polls are largely a commercial venture, this may be difficult or impossible to do.

Perhaps the evidence of the presidential election of 1948 will be found to have a salutary effect on pollsters and newspaper proprietors alike which will lead both groups to view polling results with a little more scientific caution. There is no reason, as the book under review points out again and again, to 'throw out the baby with the bath water' and assume because of one single failure of this kind that public opinion studies are worthless and ought to be abolished. Recent successes of Gallup organizations in this country, Holland, Australia and elsewhere, which have succeeded in predicting elections with 1% error or less, lend emphasis to this point.

H. J. EYSENCK

Power and Personality. By Harold Dwight Lasswell, Ph.D. (Norton). London: Chapman and Hall. 1949. Pp. vi+263. 15*s.* net.

Prof. Lasswell comes again to the attack. This time it is to investigate the effects of power in politics and the 'homo politicus' that politics throw up.

It is not a new theme. We are given a few of the many historical approaches and some dicta, which include a penetrating one of Engels that the state reflects 'the anxiety of mankind before itself'. This marches well with Freud's derivation of social conscience from 'mere dread of society'. Dreading our passionate failure before the judgement seat of fellow man, we impose the best control we can upon ourselves: but we also arrange the field to catch the slogs we must anticipate from our alter ego in action.

Prof. Lasswell introduces us to political types, the 'empire builder', the 'restorer of order', etc. The reflective 'builders of codes' also come in for mention. What then, in the context of political society, is the basis of power: Wealth, Skill, Affection? The point is that there are legion. At such superficial levels of examination we can hardly expect to find any basis of power at all. Social class certainly predisposes to political power. (An Oxford college confirms this in recently calling attention to their possessing as alumni six Prime Ministers of England during the nineteenth century.) Thus far we are still on the surface, scratching about for clues to the chosen problem: 'Whence the power and the power-lust of mankind?'

In developing his theme Prof. Lasswell does not make it easier for the general reader to follow him through the wealth of psychological jargon he employs. But there are many useful points in his description, from his own observations, of the 'extremes of indulgence and deprivation', the role of alcohol in public life, and the childish drives of some statesmen. But is there no element of 'sour grapes' when we academic folk so often conclude that 'everyone is born a politician and most of us outgrow it' (p. 160)? What about professors? Have we not all sat in chairs? We are with the author however when he asks us to increase our control of our politicians in what he calls 'decision making processes'. Until a firm core of informed common sense is available to correct the huge and disastrous errors into which certain characters will always try to lead us by virtue of their unconscious drives and towards which others will drive us with their journalistic pens, editorial ruthlessness and ideological frenzies of one kind and another, civilized mankind might appear safer if let loose again in the jungles and swamps to personal defence through the stability which emotional normality provides. But that normality itself is in itself inadequate as a guide to modern man is an argument used but not developed beyond the desirability of more self-knowledge.

Towards what? 'A freeman's commonwealth', says Prof. Lasswell. The author's idealism is as clear as is his aim of enlightenment while there is yet time. But one could wish to see a little more debunking of 'freedom for freedom's sake' in intellectual America nowadays. In a world of so many drives and so few Great Powers one wonders if it is not true of freedom as Nurse Cavell so poignantly found it of patriotism, that it is 'not enough'. That toleration of other aims and a minimum structure of order are also requisite to human salvation is an implied but hardly stated corollary of this earnest investigation of the sources of power in personality.

This book will stimulate and educate. It has copious footnotes and an adequate index.

RANYARD WEST

Psychology and Art of the Blind. By G. Revesz (translation H. A. Wolff). London: Longmans, Green and Co. 1950. Pp. xiv + 338. 42*s.* net.

The title of this book is misleading as it is neither about the psychology nor the art of the blind but concerns itself only with one aspect of each field, viz. Haptics, 'impressions conveyed by the tactile and kinematic sense', and sculpturing.

This is probably the most exhaustive study yet available in English on the usefulness and the weaknesses of the Haptic sense, although other writers (e.g. L. G. Fuchs) have been pressing for the better use of the combined tactile and kinaesthetic impression for some time. The author goes into considerable detail in discussing not only the difference between Visual and Haptic impressions but into the differences underlying the Haptics of Space, Objects and Form. In the early chapters he lays considerable stress on the care which must be exercised in examining data either from sources which can no longer be verified or from present-day experiments with blind persons because of the differences in interpretation which can occur through emphasis being laid on certain aspects of statements. In later chapters, however, he does not always make it clear whether his experiments have been done with totally blind, partially blind or blindfolded sighted people. In his view this is often not important (and certainly interpretation from sighted people using the Haptic sense appeared to differ little from that of blind people, where such information was given), nevertheless for future follow-up such information might prove essential.

In spite of the minute points of discussion entered into by Revesz it is sometimes difficult to be sure of understanding his final conclusions as, for example, on page 36 where he says 'Haptics is completely independent of the sense of vision and creates its world through its own activity and its own laws, irrespective of whether in this process it makes use of the aid of the sense of vision'.

Nevertheless, this is an important book and one that should be read not only by those interested in the various facets of psychology associated with blindness, but also those interested in the general field of Haptics. The cognitive aspect of general recognition in Haptics and individual recognition in Optics is well brought out and the difference between 'Haptomorphic' and 'Optomorphic' form discussed. Readers will be interested here to make comparison with Villey's theories. Chapter viii, though too short, gives a useful summary of the theories discussed in such detail in the first part of the book.

Part ii, in some ways, is even more interesting than the early chapters, particularly Section B, which concerns itself with a critical description of a number of blind sculptors. Those sections on Kleinhaus and Masuelli are particularly interesting.

This book may appear revolutionary to many people, particularly to educationists in the blind world. It has a valuable contribution to make to the small amount of literature at present available on the subjects with which it concerns itself. I.W.L.

The Social Function of Art. By Radhakamal Muckerjee. Bombay: Hind Kitabs Ltd. 1948. Pp. xviii + 355. Rs. 25.

This book is noteworthy for two main reasons. First, it is one of the first publications coming from India since separation, and secondly because Prof. Muckerjee has made a most gallant attempt to tackle a subject which presents such formidable difficulties that most of us who are interested in it have always postponed even beginning.

Prof. Muckerjee starts with certain fundamental assumptions about the relation of art and society. The first is that the unconscious is universal and that: 'Similarity of individual temperament and conditions of repression in social life records itself in similar myths, archetypes, symbols and images in art.' Further on he adds: 'At the same time each work of art is the revelation of the attitudes and values of a particular culture and social milieu....' Again: 'Every mature work of art expresses not only the values and life goals of a particular people but also the artist's unique vision.' These assumptions do account for the fact that one is able to appreciate the art of other culture patterns although each art will have its own specific flavour.

Recently there has been controversy whether the artist should work almost entirely in terms of his medium, subordinating ideas in fact to the physical properties of his medium, or whether he should force the medium to express the idea or the emotion. Prof. Muckerjee is in agreement with the latter point of view, which is of course that usually held by the psychologist. Here is what he says: '...art is neither an escape from reality nor a mere play with agreeable patterns of sound, shape and colour, but is essentially a revealing interpretation of certain aspects of human life and reality and the contemporary social environment.'

In a book of this nature it would be very easy to score debating points off the writer; owing to the vastness of his subject he is bound to make generalizations which cannot always be regarded as proven. For example he says: 'Myth and religion create the archetypal symbols and images....' It is more usual to reverse the process but it is not a topic on which it is wise to be dogmatic. There are a great number of generalizations in this book, many of which could be challenged in this way. Another aspect which the European reader will find difficult is the somewhat flowery language and involved sentences. However, if this is a part of one of the Indian culture patterns, as it does in fact appear to be, it is not reprehensible as it would be in an English writer.

There is much that is stimulating and valuable in this book if the reader can overcome the two difficulties mentioned above. The illustrations are delightful, though the binding will probably not stand up to the rough usage of libraries. Prof. Muckerjee is to be congratulated on his courage in attempting such an enormous and difficult subject. MADELINE KERR

Educational Psychology. By CHARLES FOX. Fourth Edition. London: Routledge and Kegan Paul. 1950. Pp. viii + 388. 21*s.* net.

This is a revision of a book which first appeared 25 years ago. The differences between this and the original edition reflect the changes in the interests of educational psychologists which have taken place during this period, though the author has not found it necessary to change his point of view, which has always been favourable to the Gestalt approach. His style of presentation is markedly individual; the style and much of the terminology have a somewhat old-fashioned flavour, but are pleasantly free from some of the modern educational jargon. Many of the investigations referred to are now of respectable age, though, of course, none the worse for that. Expressions of personal opinion are frequent: they are not all self-evident and we should have liked to see more evidence adduced for some of them.

The sub-title is 'Problems and Methods', and there is more theory than is usual to-day in text-books of educational psychology. The last two chapters (which are new) are, as the author says, synthetic and apply the theory to school situations.

There are one or two slips—e.g. Burt is quoted as saying that an I.Q. above 115 or 120 indicates modern secondary school ability at least; and some inconsistencies—e.g. the frequent use of the word *faculty*, though we are constantly being warned against the errors of the faculty psychologists. The method of citing references is irritating, requiring one constantly to turn to the back of the book to find out who said something.

To the reviewer this seems an uneven book. Some chapters are lively and suggestive, others rather turgid and unclear. There is much of value and importance for those interested in the real problems of education. Some sections may well arouse disagreement and resistance, but it should prove stimulating and thought-provoking to the serious student. A.H.D.T.

A Handbook of Employment Interviewing. By JOHN MUNRO FRASER. London: MacDonald and Evans. 1950. Pp. 202. 8*s.* 6*d.* net.

Mr Munro Fraser has produced a most readable and comprehensive volume which has long been needed. It is intended as 'a practical handbook which will be found useful by those engaged from day to day in finding the right man for the job, or in deciding how to make the best use of available manpower'. The author has combined his extensive experience with the National Institute of Industrial Psychology with his work as a vocational psychologist in the re-settlement work of our re-patriated prisoners of war, in his presentation of an exact framework for employment interviewing, all interpreted in the light of contemporary views on the value and limitations of psychological tests, and showing also the influence of the methods evolved by War Office Selection Boards in their observation of officer candidates in group and individual situations, including the interview.

With a convenient summary at the end of each chapter, the author builds a clear picture of how information can be collected about an individual, beginning with the discriminating use of application forms and leading up to a sensitive discussion of interview method and the manner in which the whole material is interpreted as a case-history from which predictions may be made. The author introduces each stage by some theoretical discussion of the psychological factors underlying the methods, such as a discussion of the meaning of intelligence and aptitude tests, and a section on motivation underlying different broad categories of interests, which will give information about the individual's general pattern of life. The whole scheme is

built around the seven-point plan of the N.I.I.P. and those categories are discussed and inter-related in some detail.

Both the long and the short interview are discussed at length, with a hypothetical example of each. An example of 'Job Study' is given, a simple one which the author chooses for purposes of clarity. But one cannot help being struck by the way in which the author uses extreme (although from his point of view probably justifiable) examples to bring his points home. This is not so much a criticism of the author, as the result of an uneasy feeling one has, that it illustrates the difficulty in presenting selection methods to the layman. It is easier, for example, with students of psychology, since one can lead up from the fairly exact general framework, to a study of the real complexity to which any attempt at understanding the individual personality brings one. Although the author devotes some space to 'The Unique Individual' it might have been a good idea to risk expanding his discussion somewhat further. He has obviously tried to be as straightforward and clear as possible, but the danger lies in the possible belief of the layman that this scheme of assessment really is as straightforward as it looks, and that he will cheerfully learn about the methods, become identified with the very straightforwardness, and resist coming to grips with the complexity. Occasional brief but misleading statements are made by the author, which overdo the simplicity, such as his comment on Freud's theories, and his inaccurate statement that 'follow-up figures indicate that methods of assessment outlined in previous chapters put the right person in the right job in about nine cases out of ten'.

As the book is designed to be essentially practical the author might have indicated how exactly information about education standards might be obtained, and he might also have included a short bibliography of elementary references.

But Mr Munro Fraser has dealt extremely well with a difficult task, and his book will certainly be very valuable also to university students of occupational psychology, however unacademic he intended it to be.

F. E. MACNEILL

Psychology and Mental Health: A Contribution to Developmental Psychology. By J. A. Hadfield. London: Allen and Unwin. 1950. Pp. 444. 18*s.* net.

Dr Hadfield writes in the spirit of the group of eclectic psychotherapists who were centred on the Tavistock Clinic in the 1930's, and appeared to be establishing a distinctive and valuable line of work. It is probably inevitable, in our present phase of regimentation and indoctrination, that this spirit should be out of fashion with its tolerance in questions of theory and its freedom from professional rigidity. All the more welcome, therefore, is a general text-book by one of the leading figures of that group.

Covering the whole field of psycho-neurotic states and behaviour disorders, the book can be recommended as an exceedingly useful introduction of the more thorough kind. Besides the elementary descriptive material needed by the young student (and illustrated by reference to a great diversity of clinical experience) it offers coherent theory and systematic thought over the whole of its range. It has the great merit that even what the reader may doubt or later come to disagree with is stated clearly and simply and invites ready comparison with other writers' views. Definitions are precise and explanatory theories are of a kind that can be tested by clinical observation, not the kind that are so 'fundamental' as to square with anything and everything that may be observed.

In discussing the aetiology of the psycho-neuroses Dr Hadfield gives as their basic cause the repressed craving for a love of which the patient has felt deprived; he insists that love and sexuality are not to be equated, and suggests that 'the need for protective love and security is of far greater importance in the development of the psycho-neuroses than the sexual'. His general standpoint is therefore closely similar to Suttie's (as is his account of specific reactions to the sense of being deprived of love), and is not far from that of Horney. In Dr Hadfield's handling of the theme the importance of the part played by repression is brought out more vividly than in other writers of the same general outlook. This theory of the aetiology of the psycho-neuroses is convincingly argued in the chapter which formulates it, but its value appears most clearly in its illumination of a great range of clinical material in the rest of the book.

The title of the book is misleading. It might sound like just one more helpful little book of popularized psychotherapy, when it is in fact a serious contribution to theory and rich in original suggestions often thrown out quite casually. The loose style of writing may also be a barrier to appreciation; the book is liberally besprinkled with misprints, mis-spellings and faulty grammar. It has all the carelessness of speech. But it is English speech, unpretentious and clear, and as such is vastly preferable to the jargon of many psychological writers.

D.W.H.

Masochism in Modern Man. By THEODOR REIK. New York: Farrar, Straus (London Allen and Unwin). 1949. Pp. vi+442. 25*s*. net.

This is a reprint of a book published in the United States in 1941, presenting Reik's theme of masochism a an expiation, in advance, of a final gratification and triumph. The discussion, though rather diffuse, thorough and subtle. What the author calls social masochism, as distinct from sexual, is given length examination. Its bearings on the Judaeo-Christian religious tradition are clearly brought out, and th discussion includes an interesting comparison and contrast between martyrdom and the simpler forms masochism.

Human Personality and its Minor Disorders. By WILLIAM HARROWES. Edinburgh E. S. Livingstone. 1949. Pp. viii+260. 15*s*. net.

This is an account of the development of human personality in its lifelong task of adaptation to the enviro ment. The first few chapters deal with fundamental mental processes and their early development, on th lines of Adolf Meyer's system. Unfortunately here the style is often laboured and confused, thus obscuri the meaning. The latter part of the book is much happier: in it, Dr Harrowes describes particular aspec of development such as social and sexual adaptation, and more specifically some of the neurotic disorde to which personality is prone. Although brief, and therefore almost inevitably somewhat superficial, the chapters are illuminated by insight and understanding, and are consequently much easier in style. T book forms a good introduction to Adolf Meyer's principles, and seems on the whole sounder psychiatrical than psychologically. A. M. SILV

The Show of Violence. By FREDRIC WERTHAM. London: Victor Gollancz. 194 Pp. viii+280. 15*s*. net.

In a style intended for non-technical readers, this book gives vivid accounts of psychiatric proble surrounding half a dozen murderers with whom Dr Wertham was professionally concerned. Some we insane, two were simulating insanity and had secured diagnoses of insanity from other psychiatris Dr Wertham makes, and offers evidence for, disturbing charges of corruption and professional incompeten against some of the forensic psychiatrists of New York, including some who held responsible positions the public service. But his main concern is for a more general understanding of the insane murderer a a more sensitive social conscience working through the legal system. It is interesting to note that he s no means at present of improving on the Macnaughton rules, though it seems evident that the Americ courts, following the interpretation of Judge Cardozo, can use them more flexibly than the English cou generally do. He regards the idea of the 'irresistible impulse' as psychiatrically unsound and socia backward.

The book is valuable to psychologists for its rather full reports of interviews with murderers after (a sometimes before) arrest, as well as its good summaries of their life histories. From the accounts of le procedures one sees what little headway an intuitive and emotionally responsive man like the author make with legal minds. The last chapter, strongly felt but not closely reasoned, extends the conceptio murder far and wide to include such deaths as those resulting from wartime blockades, industrial accide childbirth, and the economic exploitation of colonies, as well as the shorter expectation of life associa with low economic status.

The Inner Experience of a Psychoanalyst. By THEODOR REIK. London: Allen and Unw 1950. Pp. xiv+514. 21*s*. net.

This book is described as Theodor Reik's story of an expedition into the last dark continent on ea Those who attempt to keep up with this expedition may be perturbed by the apparent lack of plann and of destination, by the numerous divagations and by the leader's evident hostility towards almost of his fellow travellers.

Dr Reik is much concerned with unconscious communication and unconscious anticipation. Altho here, as elsewhere, he has many interesting observations to make, his theoretical constructs appear unne sarily extravagant, unverifiable and sometimes incomprehensible.

In the analytical session Reik would apparently agree with the Zen Buddhists that 'words and inte tion—the more with them the further we go astray'; outside it, he complies with the second imp injunction only for, in the words of Horace, 'verbaque praevisam rem non invita sequentur'. G